Vol. 44, No. 6 November, 1937

PSYCHOLOGICAL REVIEW PUBLICATIONS

# Psychological Review

EDITED BY

HERBERT S. LANGFELD

PRINCETON UNIVERSITY

## CONTENTS

*A Psychological Function is the Relation of Successive Differentiations of Events in the Organism:* EDWIN G. BORING ..... 445

*The Learning-Remembering Process—(A Reply to Professor Cason):* STEUART HENDERSON BRITT 462

*The Organization of Behavior in the Newborn Infant:* KARL C. PRATT 470

*The Place of Meaning in Psychology:* GLENN D. HIGGINSON ..... 491

*Reversal of Auditory Localization:* PAUL THOMAS YOUNG ..... 505

*On Gestalt-Qualities:* CHRISTIAN VON EHRENFELS ..... 521

*Tolman on Associative Learning:* E. R. GUTHRIE ..... 525

*H. A. Carr on the Problem of Reliability:* WALTER S. HUNTER ..... 529

*A Note on Verifications of the Judgmental Theory of Pleasantness and Unpleasantness:* HENRY N. PETERS ..... 533

PUBLISHED BI-MONTHLY
FOR THE AMERICAN PSYCHOLOGICAL ASSOCIATION
BY THE
PSYCHOLOGICAL REVIEW COMPANY
PRINCE AND LEMON STS., LANCASTER, PA.
AND OHIO STATE UNIVERSITY, COLUMBUS, OHIO

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879

# PUBLICATIONS

OF THE

# AMERICAN PSYCHOLOGICAL ASSOCIATION


EDITED BY

S. W. FERNBERGER, University of Pennsylvania (*J. Exper. Psych.*)
WALTER S. HUNTER, Brown University (*Abstracts*)
HERBERT S. LANGFELD, Princeton University (*Review*)
JOHN A. McGEOCH, Wesleyan University (*Bulletin*)
JOHN F. DASHIELL, Univ. of North Carolina (*Monographs*)
GORDON W. ALLPORT, Harvard University (*J. Abn. and Soc. Psychol.*)

WILLARD L. VALENTINE, Business Editor


## PSYCHOLOGICAL REVIEW

containing original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 540 pages.

## PSYCHOLOGICAL BULLETIN

containing critical reviews of books and articles, psychological news and notes, university notices, and announcements, appears monthly (10 numbers), the annual volume comprising about 720 pages. Special issues of the Bulletin consist of general reviews of recent work in some department of psychology.

## JOURNAL OF EXPERIMENTAL PSYCHOLOGY

containing original contributions of an experimental character, appears monthly (beginning January, 1937), two volumes per year, each volume of six numbers containing about 625 pages.

## PSYCHOLOGICAL INDEX

(discontinued after No. 42, June, 1936)

is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The Index has been published annually from 1895 to 1936, the volumes covering the literature of 1894 through 1935. Its bibliographical services will be carried on by the Psychological Abstracts.

## PSYCHOLOGICAL ABSTRACTS

appears monthly, the twelve numbers and an index supplement making a volume of about 700 pages. The journal is devoted to the publication of non-critical abstracts of the world's literature in psychology and closely related subjects.

## PSYCHOLOGICAL MONOGRAPHS

consists of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. The Monographs appear at irregular intervals and are gathered into volumes of about 500 pages.

## JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY

appears quarterly, June, September, December, March, the four numbers comprising a volume of 560 pages. The journal contains original contributions in the field of abnormal and social psychology, reviews, notes and news.

## ANNUAL SUBSCRIPTION RATES

**Review:** $5.50 (Foreign, $5.75). **Journal:** $14.00 ($7.00 per volume, Foreign $7.25).
**Monographs:** $6.00 per volume (Foreign, $6.30).
**Bulletin:** $6.00 (Foreign, $6.25). **Abstracts:** $7.00 (Foreign, $7.25).
**Abnormal and Social:** $5.00 (Foreign, $5.25). Single copies $1.50.
**Current numbers:** Journal, $1.25; Review, $1.00; Abstracts, 75c; Bulletin, 60c.

## COMBINATION RATES

**Review and Bulletin:** $10.00 (Foreign, $10.50).
**Review and J. Exp.** (2 vols.): $17.00 (Foreign, $17.75).
**Bulletin and J. Exp.** (2 vols.): $18.00 (Foreign, $18.75).
**Review, Bulletin, and J. Exp.** (2 vols.): $22.00 (Foreign, $23.00).

Subscriptions, orders, and business communications should be sent to the

## PSYCHOLOGICAL REVIEW COMPANY
## OHIO STATE UNIVERSITY, COLUMBUS, OHIO

*PSYCHOLOGICAL REVIEW PUBLICATIONS*

# Psychological Review

*EDITED BY*

HERBERT S. LANGFELD, PRINCETON UNIVERSITY

S. W. FERNBERGER, UNIV. OF PENNSYLVANIA (*J. of Exper. Psychol.*)
W. S. HUNTER, CLARK UNIVERSITY (*Psychol. Index*)
JOHN A. McGEOCH, WESLEYAN UNIVERSITY (*Psychol. Bull.*)
JOHN F. DASHIELL, UNIVERSITY OF NORTH CAROLINA (*Psychol. Monog.*)

## VOLUME XLIV, 1937

PUBLISHED BI-MONTHLY
FOR THE AMERICAN PSYCHOLOGICAL ASSOCIATION
BY THE PSYCHOLOGICAL REVIEW COMPANY
PRINCE AND LEMON STS., LANCASTER, PA.
AND PRINCETON, N. J.

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa. under Act of Congress of March 3, 1879

LANCASTER PRESS, INC., LANCASTER, PA.

# CONTENTS OF VOLUME 44


ADAMS, D. K., Note on Method ..... 212–218
ALDRICH, V. C., A Theory of Ball-Play ..... 395–403
ANGIER, R. P., Edward Stevens Robinson ..... 267–273
BELLOWS, R. M., An Experimental Isolation of Some Factors Determining Response to Rhythmic Cutaneous Stimulation. III. Interpretation ... 62–76
BORING, E. G., A Psychological Function is the Relation of Successive Differentiations of Events in the Organism ..... 445–461
BRITT, S. H., The Learning-Remembering Process—(A Reply to Professor Cason) ..... 462–469
CARR, H. A., The Search for Certainty ..... 274–296
CASON, H., The Concepts of Learning and Memory ..... 54–61
VON EHRENFELS, C., On Gestalt-Qualities ..... 521–524
EWERT, H., Factors in Space Localization During Inverted Vision ..... 105–116
GENGERELLI, J. A., The Dichotomy of Science and Philosophy ..... 117–137
GIBSON, J. J., Adaptation with Negative After-Effect ..... 222–244
GUTHRIE, E. R., Tolman on Associative Learning ..... 525–528
HEVNER, K., The Aesthetic Experience: A Psychological Description ..... 245–263
HIGGINSON, G. D., An Examination of Some Phases of Space Perception ... 77–96
——, The Place of Meaning in Psychology ..... 491–504
HOLT, E. B., Materialism and the Criterion of the Psychic ..... 33–53
HULL, C. L., A Comment on Dr. Adams' Note on Method ..... 219–221
——, Mind, Mechanism, and Adaptive Behavior ..... 1–32
HUMPHREY, G., A Note on System-Theory ..... 346–348
HUNTER, W. S., H. A. Carr on the Problem of Reliability ..... 529–532
KRECHEVSKY, I., A Note Concerning 'The Nature of Discrimination Learning in Animals' ..... 97–104
LEWIN, K., Carl Stumpf ..... 189–194
LOUCKS, R. B., Reflexology and the Psychobiological Approach ..... 320–338
MAIER, N. R. F., Reasoning in Rats and Human Beings ..... 365–378
MASLOW, A. H., Dominance-Feeling, Behavior, and Status ..... 404–429
PETERS, H. N., A Note on Verifications of the Judgmental Theory of Pleasantness and Unpleasantness ..... 533–535
PRATT, K. C., The Organization of Behavior in the Newborn Infant ..... 470–490
PRICE, B., A Note on Professor Tryon's Theory of Intercorrelation ..... 183–187
RAY, W. S., The Relationship of Retroactive Inhibition, Retrograde Amnesia and the Loss of Recent Memory ..... 339–345
RUCKMICK, C. A., Psychology Tomorrow ..... 138–157
SCHLOSBERG, H., The Relationship Between Success and the Laws of Conditioning ..... 379–394
SEWARD, G. H., AND J. P., Internal and External Determinants of Drives ... 349–363
SHUEY, H., The Fundamental Principles of Typology ..... 170–182
SPENCE, K. W., The Differential Response in Animals to Stimuli Varying within a Single Dimension ..... 430–444

TOLMAN, E. C., **Demands and Conflicts** ..... 158–169
——, **The Acquisition of String-Pulling by Rats—Conditioned Response or Sign-Gestalt?** ..... 195–211
WEBER, P. L., **Judgment Today** ..... 264–266
WENGER, M. A., **A Criticism of Pavlov's Concept of Internal Inhibition** ..... 297–312
YOUNG, P. T., **Is Cheerfulness-Depression a General Temperamental Trait?** 313–319
——, **Reversal of Auditory Localization** ..... 505–520

VOL. 44, No. 6 NOVEMBER, 1937

THE PSYCHOLOGICAL REVIEW

# A PSYCHOLOGICAL FUNCTION IS THE RELATION OF SUCCESSIVE DIFFERENTIATIONS OF EVENTS IN THE ORGANISM

BY EDWIN G. BORING

*Harvard University*

The trouble with the concept of cause-and-effect is Newton's third law. The reaction of the effect upon the cause matches the action of the cause upon the effect, so that we have, not a serial progressive action, but that mutual interdependence of two events which makes them into a system. Everything is interrelated in this way. The universe is a system, and to partition it into part-systems is a falsification. If a man is ever to utter the whole truth about a natural event, he must not shut his mouth until he has expressed all nature.

Nevertheless man has perforce to be content with much less than that mouthful. He describes events within limits, and he succeeds again and again in distorting them only by an amount that is negligible. There are practical lateral and terminal limits that can be set to events. One can think of a neuron as a system and not commit any great error. Of course, the neuron is not insulated laterally, for its recovery after excitation is dependent upon the circulation, and the restorative power of the blood depends upon nutrition, and nutrition upon food, and food upon food supply, and food supply upon economic legislation, and the legislation upon world politics, and so on. This extension of the neuron-system is not imaginary: the nerves of the man who is dead from economic want do not react as they did in prosperity. Similarly we fix the terminal limits of a neuron-system at the synapses, although it is probable that even there some

reaction occurs to alter the action. Even though systemic limitation may be arbitrary and only approximate, no one will deny its practical necessity nor the fact that we may sometimes make accurate scientific progress by accepting the fiction that a system can be isolated from the rest of the world.

It would seem that these fictitious systemic boundaries can be interpolated successfully when there is great disparity between the events in adjacent systems, and in some such cases we can actually recover for practical use the convenient relation of cause-and-effect which the universality of systemic relations had caused us to abandon.

Sometimes this disparity is temporally from the great to the small. The big event has a small effect. The effect then reacts upon the event, but its reaction is negligible to the event. Thus approximately we have one-way action. Take the series of miniature systems affected when a sound from a loud-speaker excites the organ of Corti. The diaphragm (1) of the speaker sets up a vibratory pattern in the air (2), which by way of the tympanic membrane moves the ossicles (3), which through the oval window set up a resonating pattern in the cochlea (4), which, moving the hairs of the hair cells, arouses the electro-chemical disturbance known as the cochlear response (5), which excites the VIIIth nerve (6), which excites the lateral lemniscus (7), and so on. We do not know much about reaction at a synapse, but it is plain that there must be some reaction back from the cochlear response to the speaker diaphragm. The electrical energy dissipated in the cochlear response must increase the resistance to movement of the organ of Corti (just as it is harder to turn a generator when the switch in its output circuit is closed and current can flow); and the increased resistance of the organ of Corti must diminish the movement at the oval window, at the tympanic membrane, in the air, and at the speaker diaphragm. It is conceivable that the vibration of a loud speaker might be diminished because a man fifty feet away turned his attention to the sound! The point is that in the first five of these systems there is a disparity between the events: reaction affects the antecedent

system only a little because the antecedent system is relatively large. Destroying the apical tip of the cochlea does not greatly alter the action of the tympanic membrane for low tones.

On the other hand, the disparity is sometimes from the small to the great. This is the case of the release mechanism, the case that is typical for the nervous system. Now the antecedent system simply pulls the trigger or throws the switch, as it were, and there results an effect that is huge with respect to the energy derived from the preceding system. The magnitude of the effect depends, of course, upon energy resident in the consequent system. There must be reaction here too, but it is only of the order of magnitude of the energy required to trip the switch and is therefore negligible.

For these reasons it becomes possible to understand many psychological events as if they were cause-and-effect relations between serial systems in the living organism. Even though theoretically there is only one system, the universe, we can make progress by means of the fiction that the organism is a system separated from the rest of the universe, that the nervous system can sometimes be regarded apart from the total organism, and that portions of the nervous system can also be separated from each other for the purpose of studying their correlative relations.

## The Organism as a Systemic Series

It is the purpose of this paper to depict the organism as a series of open communicating systems, transmitting action in the direction of stimulus to response. Fig. 1 is the paradigm. Partial systems of the kind that can be separated practically from the rest of the universe can appear in any one of three ways. Such a system may be *closed*, that is to say, it may represent an equilibrium that is maintained essentially independently of events outside the system. The system may be *incomplete* in the sense that it cannot be independently described, and so must be enlarged. Or a system may be *open* in that it communicates with antecedent or consequent systems in such ways that reaction is negligible in comparison

with action. Successive neurones with synaptic communication are in this sense open systems. An electrical network may form an open system. It is a system because the relation of the current in the various parts cannot be determined by any simple logic of cause-and-effect but only by the solution of a set of simultaneous equations. On the other hand, such a system *B* can communicate with an antecedent system *A* in the sense that *B* derives its initial input voltage as the output of *A*; or *B* can communicate with a consequent system *C* in the sense that some difference of potential within *B* may become the input of *C*. Ordinarily such electrical systems would be incomplete and would have to be combined, but their separation might properly be made if, for example, the antecedent system were a compound-wound generator which approximately maintains its voltage whatever current is drawn from it, and the subsequent system were a vacuum-tube voltmeter which does not appreciably reduce a voltage in measuring it. The open systems of the nervous system tend to show relationships of this sort.[1]

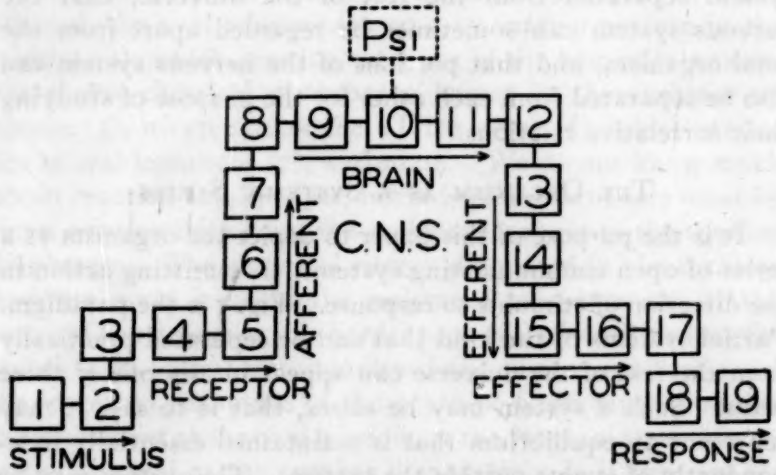


FIG. 1. Schematic diagram of the organism as a series of communicating systems.

The meaning of Fig. 1 will become clear if we apply to it the auditory illustration of the preceding section. Suppose

[1] These systems and the electrical analogy are described in (3, esp. 566–570).

that an organism presses a key which mechanically moves a signal when the organism hears a change in the pitch of a tone given by a loud-speaker. In the list below we can place tentatively successive systems with numbers corresponding to the numbers of Fig. 1.

1. Loud speaker
2. Air in room
3. Ossicles
4. Cochlea
5. Cochlear response
6. VIIth nerve
7. Lateral lemniscus
8. Aud. radiations
9. Cortical system
10. Another system?
11. Another system?
12. Motor area
13. Pyramidal tract
14. Efferent nerve
15. End-plates
16. Muscle
17. Finger
18. Key
19. Signal

The list is schematic and suggestive only. There must be other systems that should be introduced. Some of these systems, like the cochlea and the ossicles, are perhaps so incomplete that they need to be combined.

Of course the great omission from the table is the *Aufgabe*, the 'switcher.' An organism making such a specific response is following an instruction or acting in accordance with an attitude. An instruction, given as the meaning of words, is an afferent series that may come along various routes (visual, auditory; English, German) but ends up in the same excitation, since the meaning is the same. Somewhere in the region 8–12 this excitation determines what set of efferent patterns will be discharged (13–19) as the response to the stimulus (1–7). An attitude persists in the region 8–12 as a trace of the organism's past. So does every other available trace or memory. The tremendous complication of the fact is well known. Nevertheless the simple scheme of Fig. 1 serves as the paradigm to show how the organism can be regarded for the purposes of psychology.

## Isomorphic Relations

When an event is transmitted through a systemic series like Fig. 1, it sometimes remains with its essential form unchanged in successive systems. It is then possible to say that the successive phases of the event are isomorphic with each other.

Transmission across a synapse may be isomorphic since, under the all-or-none law, neural excitations are pretty much alike. The general rule is that, if two successive systems have the same properties and the output of each is like the input, then transmitted events should be isomorphic. Thus the mechanical events of the first four systems listed for hearing are anisomorphic, for the properties of the systems change and output is not like input (3, 572–579).

In general when isomorphic relations between systems are known to exist, there is no need for the word. The term is more often applied as a hopeful hypothesis in the face of insufficient knowledge of the properties of the systems involved.

## Discrimination

Discrimination is the psychical function of the organism. It is the criterion of mind, of consciousness, of knowing. Animals, children and irresponsible adults are recognized as conscious only as and in as far as they discriminate, that is to say, as they react differentially (discriminatorily) to a differentiated situation. The contents of the unconscious mind can be asserted to exist only in as far as they appear in differential response. Even the 'immediate, private experience' of introspection can be defined only operationally, and the operation which indicates its existence is differential reaction. The mere fact of psychical relativity shows that the contents of mind have no meaning apart from differentiation. If color were constant, there could be no awareness of color. It is only as color is differentiated that it provides a possibility of differential reaction. If color and size always varied together, they would never be recognized as different. Differentiation among the attributes is got by their independent variability. The psychophysical judgment is always relative. Either a standard is given as a frame of reference, or else—as in the so-called 'absolute' judgments—the frame of reference is constituted by the traces of immediately past judgments. This point has been argued elsewhere [2] and does not need extended

[2] See, for instance, (5), (6, esp. 94–98), and (2, esp. 240–244). An earlier phase of this development appears in (1, 229–233).

consideration here. The fact is that all the soluble problems of psychology can be reduced to problems of differentiated reaction if one wants so to reduce them.[3]

The only persistent opposition to the view that the subject-matter of psychology is the discriminations that organisms make comes from those persons who wish to maintain that there is scientific significance to the concept of immediate (direct phenomenal) experience. These persons are answered by the fact that even private experience is not immediate. There is not even a private meaning to a sense-datum, except as it is given in relation to some extrinsic frame of reference.

## Successive Differentiation in the Nervous System

The term *discrimination* is, however, not the most fortunate. It is a functional term representing the dependence of a consequent differentiation upon antecedent differentiation, a relation such that the later differentiation indicates the existence of the earlier. Only in this descriptive sense is the word useful. Unfortunately it tends to pick up a 'subjective' or 'dynamic' context, partly because *discrimination* has long meant a capacity which belongs to a world of private or sensory experience, and partly because dynamic or functional concepts are intuited as 'energetic,' whereas in scientific description they are nothing more than relationships. We get rigorous description only by avoiding the 'anthropomorphism' of 'dynamic' psychology, and the word *discrimination*, as it enters into this discussion, does not mean that the organism as a whole is doing the discriminating. The cochlea 'discriminates' among events in the middle ear when it responds differentially to the action of the ossicles. The VIIIth nerve 'discriminates' as to what goes on in the organ of Corti, and efferent events may be 'discriminations' of afferent events. It is therefore probably best to abandon the word *discrimination*, which implies a freely acting, conscious observer, and to limit ourselves to the descriptive terms of successive

[3] E. B. Holt (4) holds a similar position, although he singles out verbal-motor processes as characterizing consciousness. See esp. 42f., 47f.

differentiation and the relations between them. It is these relations between differentiations that are the subject-matter of psychology.

It is obvious that the differentiation of events within any one of these seriated systems of Fig. 1 is a joint function of (*a*) the properties of the system and (*b*) the differentiation of the antecedent communicating system. Three things can happen as an event is transmitted through the series.

(1) Additional differentiation may be *gained* as a result of the properties of the system in which it first appears. For instance, the mechanism of the middle ear may 'add' a difference tone to a complex of two tones, that is to say, the system of the ossicles distorts the transmitted wave-form, so that resonance to it will now yield three tones instead of two. In the same way, when the two dimensions of the tonal stimulus (frequency and intensity) yield a four-fold differentiation in the organism (pitch, loudness, volume and density), it is plain that the final differentiation reflects some properties of the transmitting chain as well as properties of the stimulus. If the four attributes are initially dependent, as Stevens has suggested, upon the differentiation of the pattern of excitation in the cochlea, then the differential observation of these attributes can be regarded as an observation of the properties of the cochlear mechanism.[4] So much has been said about the stimulus-response relation being the organism's discrimination of its environment, that it is high time to remember that a great deal of what counts for error on the part of the organism in discriminating its environment is really its correct perception of properties of its own self.

(2) On the other hand, differentiation may be transmitted part of the way and then *lost*. There is a great deal of differentiation in the stimulus which has negligible effect upon the organism, like the polarization of light and the phase relations of tones heard monaurally. There must also be

[4] See (2, esp. 240–245). Stevens' suggested correlation of the tonal attributes with the pattern of excitation in the organ of Corti is: pitch—place of maximal excitation along the basilar membrane; loudness—number of nerve fibers excited; volume—spread of excitation along the basilar membrane; density—number of fibers excited per unit of spread.

many incidental properties of the parts of the nervous system that do not get passed on. It seems probable that the frequency of a tonal stimulus below 1000 c.p.s., although recoverable from the VIIIth nerve, is lost at higher levels of the auditory tracts. In such a case the frequency would be incidental to the physics of transmission but would have no further significance in the psychic life of the organism.

(3) There remains the case in which differentiation is *maintained* and thus transmitted. Although some psychologists are interested in such relations as the adequate response of the cochlea to the auditory stimulus, psychological problems for the most part are based upon overt differentiation that appears in effector mechanisms—behavior, both concrete and verbal. Any discriminatory behavior that reflects adequately the properties of the stimulus means that differentiation in the stimulus has been carried all the way through the stimulus-response series. Sometimes differentiation in the response reflects a property of a phase of the train subsequent to the stimulus, as would be the case if a judgment of tonal volume is dependent upon the spread of excitation in the organ of Corti. In other words, there is always a problem as to where the final differentiation originates within the organism or before the organism. Any judgment that depends upon learning is a differentiation in response that has originated at some post-afferent point, for to learn something is to change the properties of the brain.

These three cases represent three fundamental psychological problems. Is one 'conscious' of the stimulus, or of the processes in the sense-organ, or of the engrams in the brain? The *Aufgabe* and attitudes introduce still other problems, of course, but it is already clear that it is important to know where differentiation originates in the organism, when it is known to appear in response. The place of origin of the differentiation determines, at least in the common-sense meaning of terms, *what* it is that is being discriminated.

There is no possibility that, by observing stimulus and overt response at the organism's periphery, we should ever be able to localize properties within the organism. If something

comes out that did not go in, who shall say where it entered the series? We can find out only by tapping into the series at some point. The discovery of the method of amplification of action potentials has taught us a great deal about auditory stimulation. We can tap the series by observation at the hair cells, at the VIIIth nerve, at various higher levels in the auditory tract. Mechanical interference with the cochlea gives us more information, and physical methods enable us to know much that goes on outside of the tympanic membrane. Observation of peripheral nerves shows us that the differentiation in the response must be fixed at some pre-efferent level. The *Aufgabe* as a switching mechanism must act in the brain, and what the *Aufgabe* affects in the response could not be added in the efferent tracts. There is then considerable advance in the localization of these special properties in the nervous system, but this knowledge could never have been gained except by breaking into the stimulus-response series with a new method of control (*e.g.*, *Aufgabe*) or of observation (*e.g.*, action potential).

## Psychoneural Isomorphism

The foregoing argument contains the lethal objection to the theory of psychoneural isomorphism, and indeed to any parallelistic dualism of consciousness and brain. Fig. 1 represents consciousness (phenomenal experience) as *Psi*, paralleling the brain system, no. 10. Such is the usual assumption of parallelism. Psychoneural isomorphism assumes that certain essential differentiation in *Psi* corresponds to similar differentiation in 10. There are various ways in which we can learn about the neural system 10, but there is no way of learning about *Psi* except by introspection, and introspection is differentiated response in the 15–17 region (Fig. 1), and can reflect differentiation in *Psi* only as it is transmitted through 10. Hence only those events in *Psi* which are represented by events in 10 could ever be known. We must conclude that, if there were a *Psi* paralleling 10, then the only events in *Psi* that could ever be known would be those that are isomorphic with events in 10. We see here why psychoneural isomorphism

is plausible. However, the theory is not only plausible; it is also untenable, for there is no possible way in which *Psi* can be given a separate existence from 10, since all information about *Psi* must pass through 10. If we could put an electrode on the 'psychoneural joint' between *Psi* and 10, in order to find out what originates in *Psi* and what in 10, then—and then only—should we have the opportunity of proving or disproving psychoneural isomorphism.[5]

## Consciousness

An objective theory of the mind, such as this paper presents, does not reject consciousness! Let us get that matter straight. Instead it ingests consciousness. Its chief value is that it provides so good a definition of consciousness that the rigor of the definition turns out to be preferable to the uncertainty of the term. Thus we have the paradox that a system invented to account for consciousness seems to get along without consciousness. There is, however, another reason for abandoning this precious word.

*Consciousness* as traditionally used is an ambiguous term, as is also every other term applied to 'immediate,' 'direct,' or 'phenomenal' experience. The two definitions of consciousness are:

*A. Consciousness = awareness of an object.*
*B. Consciousness = awareness of the awareness of an object.*

Let us see to what sort of psychological events or relations these two different concepts apply.

## Consciousness as Awareness of an Object

To define consciousness as awareness of an object is to equate consciousness to discrimination, and thus—in the terms that this paper sets down—to the relation of differentiation in response to differentiation in stimulus. The man who says "I see green," and who in such a saying consistently

[5] See (3, esp. 574–586). I wish I could substitute the paragraph above for pp. 582–586 in this article, for I think that I have now got the matter stated more clearly and succinctly.

reflects a variable property of the stimulus, is conscious of the green in the stimulus, because he is responding adequately to it. This is what most laboratory introspection is, and it would seem therefore to be a useful definition of consciousness. The difficulty is that the definition includes so many other cases that are not ordinarily regarded as examples of consciousness.

Under this definition *A* the following events would all be conscious. (1) The recognition of a familiar face among strange faces, an instance which would be disputed by none. (2) Looking toward the door when there is a knock at it. (3) Eating raisins while absorbed in conversation after dinner. (4) Pronouncing the word *the* as it occurs casually in the middle of a glib *ex tempore* speech. (5) Winking when the eye-ball is dry. (6) Continuing to swallow when food is half-way down the œsophagus. (7) The phototropic turning of the protozoan toward the light. (8) The movement of the iron filings toward the magnet. We can paraphrase these situations. For instance, we can say: 'he knows that the raisins are there though he is too absorbed to notice them;' 'he has not time to choose his words, but his voice chooses for him;' 'the eyelid knows that the eye is dry;' 'the iron filings know where the magnet is.' The list runs from the discriminatory relations that are ordinarily called conscious, down through the habituations and automatic actions which are supposed to be less conscious, on through the tropisms which have for forty years been only hesitantly classified as conscious, to the inorganic relations which differ in no whit from the conscious ones but which are never called conscious.

Here then lies the first difficulty with this concept of consciousness. Persons who accept the definition for the top of the hierarchy are unwilling to go through with it to the bottom. Yet the series is continuous and there is no essential difference between the human and the inorganic pattern. We are meeting here with the conventions of usage and not with logic. It is obviously best to abandon the word *consciousness* in the sense of awareness of an object and to deal instead with

relations between systemic differentiations where there can be no dispute about terms.[6]

## Consciousness as Awareness of Awareness of an Object

The reason why people will accept definition *A* as meaning consciousness in man and reject it as meaning consciousness in iron filings is that they have been trained from an early age to think of consciousness as an essential attribute of man, a probable attribute of animals (especially vertebrates), an improbable attribute of plants, and an impossible attribute of inorganic things. Definition *B*, however, practically limits consciousness to man and is therefore likely to be more acceptable. Certainly there are many cases in which consciousness is attributed to a response solely because the organism is conscious of being conscious of an object.[7]

The crucial instances here lie in automatic behavior. Take the case where the eye runs down columns of proper names in order to note all the *Smiths*. The names, we shall suppose, are arranged in random order. The inspection can be rapid and yet no errors be made. Not a single *Smith* is missed. Is every other name, therefore, perceived as *not-Smith?* Yes, in the sense of definition *A*. To be correct in not stopping at a name that is not *Smith* is to react adequately and differentially to that name, to perceive the name, to be conscious of it in the sense of *A*. Yet most persons would prefer not to attribute consciousness to these responses where there can be no recall or recognition of the non-crucial names. What is the difference between the *Peterson* that goes unnoticed and the *Smith* that is noticed? It is possible for the organism to react correctly to his reaction to *Smith;* he knows

[6] *Cf.* here Holt's citation of P. Souriau: "Vision is an absolutely unconscious act," and "Sensations are, therefore, by their very nature unconscious" (4, 39–42, esp. 40). His argument is similar to mine. In general let me say that I have always owed Holt a greater intellectual debt than I have ever been able to acknowledge by citation of article and page in his writings.

[7] Again *cf.* Holt (4, 40), who rejects this regress: "Thinking and knowing are processes in their own right: to think is not to think that I think that I think; and to know is not to know that I know that I know—into all eternity. In short, these considerations convey the gentle hint that philosophers and psychologists might improve themselves by learning not to stammer."

that he reacted; he is aware of having been aware. No such statements can be made about *Peterson*. The best that the organism can do about *Peterson* is to read the list slowly again, single out the name, and infer that he must have reacted to the word because he was correct in not noting it.

It would be possible to develop this conception through a great many intricacies that would not be worth our exploration. The self-conscious introspector is aware that he is aware that he is aware of the object. By empathy one organism is aware at once that another organism is aware of an object. An animal reacting to his reaction would come under this definition. Yet altogether the definition marks off a class of mental events that are normally human, and it serves for the human being to differentiate the conscious from the unconscious.

Of course the unconscious, in so far as it can be known, is dependent upon differential response for its revelation. Definition *A* would make the unconscious conscious; but definition *B* preserves the distinction between the conscious and the unconscious. When a person acts in accordance with a repressed idea without knowing the reason for his action, he is said to be motivated by the unconscious: he is aware of something (definition *A*: adequate response), but he is not aware that he is aware (definition *B*). But, when the motivating idea is fully conscious, then he is aware of the something, and aware that he is aware of the something. On this second definition of consciousness depends the important distinction between the conscious and the unconscious.

We have to understand this second definition of consciousness in order to realize what has become of consciousness in the objective system that this paper sets forth. Having understood, tough-minded rigorous thinkers will, I think, want to drop the term *consciousness* altogether. A scientific psychology is scarcely yet ready to give importance to so ill defined a physiological event as an awareness of an awareness. This concept might never have come to the fore had not people tried to interpret others in terms of their own 'private' minds—that egocentric Copernican distortion which properly leads to desolate solipsism.

## Private Experience

Let us now ask ourselves the direct question: Have we, by explaining consciousness out of existence, by transforming it into a physical relation as we do when we give it its rigorous operational definition, have we by such an amœboid ingestion of consciousness proved that private experience does not exist? I think the answer is clear: Private experience does not exist, in any useful meaning of the term existence (4, 46f.).

The term is a contradiction. Perhaps, when an atom of oxygen woos two atoms of hydrogen, its need appears in a consciousness that is over and above the fact of the dynamic relation between hydrogen and oxygen; but, if that is so, such consciousness is truly private. It never becomes public, and for all practical purposes of science and philosophy it does not exist. The human consciousness for which common sense asks our belief can have useful meaning for us only in so far as it is *not* private.

Nevertheless common sense still meets us with the assertion that private experience is the obvious ineluctable certainty of every person. "You do not mean to tell me," it says, "that you stand there, talking to me, without being intimately aware of me, without a complement of perceptual experience that is immediately your own before you even begin to think how to impart knowledge of its nature to anyone else? A denial of private experience is a *reductio ad absurdum*." The answer to this demand is more than Yes. It involves the demonstration that the person who is so sure of his immediate awareness or private experience, nevertheless has difficulty in determining just when he has been aware or has had experience. When he rapidly scrutinizes a set of visual objects to pick out a familiar one, and thus successfully ignores all the others, was he aware of every one of the others in order correctly to ignore them? We have noted above that he must be aware of the others, but not aware of his awareness. He himself will be puzzled to say of what he was aware. There is nothing obvious and certain to this 'experience' whose owner has to reason out its existence just as the public might do. Awareness is not necessarily immediate or private. A doubt occurs as to where the

boundary of privacy is to be drawn, and, if some 'private' experience can be attested only by an inference that might as well be public, then the whole concept becomes shaded with doubt.

Still the stubborn man of common sense will say: "But some experience at least is immediately given. To have it is to know that you have it." To this I can only say that I do not find the world thus. My private experience is always an inference as to what is just past—in the last minute or the last half-second. That experience can also be the knowing of it does not make sense. Moreover, with all these contradictions popping up about immediate and private experience, I think the burden of proof lies with the person who insists on the existence of ineffable private experience. I feel sure that an intimate facing of the facts would reduce all personal experience to retrospection and inference of the kind that can be shared publicly.

In any event—and this is my *coup de grâce* to privacy of experience—can there be a useful meaning given to any private experience that does not possess the property of potential publicity? The experience can be, and in science must be, published, and the operation of publication is physiological and therefore physical, so that the means of publication come to provide a physical operational definition of experience. In this way experience inevitably loses its privacy, its immediacy and its nonphysical character. Nor do I find anything in introspection to confound this conclusion.[8]

## Summary

For psychological purposes the organism can be regarded as a series of communicating systems in which the action is in the afferent-efferent direction. Causal interpretations can without undue falsification be applied to the relations between these systems, if the systems are properly chosen.

Discrimination, the psychic function of the organism and the criterion of psychological events, can be understood as the

[8] Privacy tends to reduce to the alleged self-validation of consciousness, the view that it is "impossible for any one to perceive, without perceiving that he does perceive" (Locke). *Cf.* Holt on the persistence of this dogma (4, 33–35).

differentiation in a consequent system that adequately reflects the differentiation in a prior system. Differentiation can be begun, continued or ended at any point in the nervous system, and one of the primary tasks of psychology is to determine the place of origin of such differentiation as issues in overt response. Only by entering the systemic series by other observational methods can these places of origin be determined. For this reason psychoneural isomorphism turns out to be a meaningless concept, since there is no method by which the psyche can be differentiated from the brain.[9]

*Consciousness* is an ill-defined, equivocal term. It may mean adequate discrimination, but this definition includes inorganic relations as well as animal and human. It is sometimes used in the sense of awareness of awareness (reaction to a reaction) and the distinction between the conscious and the unconscious is of this kind. On the other hand, both kinds of consciousness are explained and the use of the term *consciousness* is avoided, if we speak more rigorously in terms of the dependence of the differentiation in one neural system upon the differentiation in another. With such a set of simple physical concepts we avoid the danger of founding a scientific account of mind upon these vague intuitions of the nature of private consciousness, which common sense has stamped upon the intellectual predilections of every man.

## REFERENCES

1. Boring, E. G., The physical dimensions of consciousness, New York: Appleton-Century, 1933.
2. ——, The relation of the attributes of sensation to the dimensions of the stimulus, *Philos. Sci.*, 1935, 2, 236–245.
3. ——, Psychophysical systems and isomorphic relations, Psychol. Rev., 1936, 43, 565–587.
4. Holt, E. B., Materialism and the criterion of the psychic, Psychol. Rev., 1937, 44, 33–53.
5. Stevens, S. S., The operational basis of psychology, *Amer. J. Psychol.*, 1935, 47, 323–330.
6. ——, Psychology: the propaedeutic science, *Philos. Sci.*, 1936, 3, 90–103.

[MS. received May 14, 1937]

[9] This argument is closely related to the contention that the concept of psychophysical parallelism is self-contradictory, because mind and brain cannot be distinguished unless they can be shown to vary independently, whereas psychophysical parallelism asserts that they do not vary independently although they are distinguished.

# THE LEARNING–REMEMBERING PROCESS— (A REPLY TO PROFESSOR CASON) [1]

BY STEUART HENDERSON BRITT

*The George Washington University*

The concepts of learning and memory as discussed by Professor Hulsey Cason [2] seem to the present writer to need reconsideration. We agree with Professor Cason that the concept of learning is broader than all of the processes (of the nature of learning) observed in any one animal species or in any limited range of human ages. We agree that the concept of learning is broader than any one of the several existing forms of learning (*e.g.*, the conditioned response, paired associates, redintegration, multiple choice, the association of ideas, etc.). And of course we concede that, "An acceptable definition of learning should include those psychological activities which it is desired to include, and exclude those which it is desired to exclude" (p. 55).

However, the four definitions relating to the problem of learning merit careful analysis:

Learning is the establishment or strengthening of neural connections between stimulating processes and responding processes as a result of accompanying or immediately preceding psychological activities (p. 55).

Retention is the partial or complete persistence of learned connections between stimulating processes and responding processes (p. 59).

Forgetting is the partial or complete loss of learned connections between stimulating processes and responding processes (p. 60).

Reproduction is the revival of responding processes as a direct and immediate result of the action of stimulating processes, the

[1] Acknowledgment is made of the friendly criticism of Professor Marion E. Bunch, of Professor Mitchell Dreese, and of Dr. H. M. Johnson, none of whom, however, is to be held accountable for specific views expressed.

[2] H. Cason, The concepts of learning and memory, PSYCHOL. REV., 1937, 44, 54–61.

connections between the two kinds of processes having been previously learned (p. 60).

Our reconsideration of these definitions is not motivated by an attack on the precise words used in those definitions; reasonable men may disagree as to refinements of word nuances. Rather, there are at least ten fundamental *points of view* expressed by Cason in his discussion of these definitions with which we differ:

1. Throughout the article in question, 'learning' and 'memory' are used as nouns which correspond to entities—real things. This is the error of reification. Much confusion would be avoided if it were indicated that these nouns are always to be thought of as verbs or adverbs; for example, instead of 'memory,' we should properly say, 'remembering' or 'somebody remembers.'[3] Learning and remembering are *activities* or processes, and this point of view affects the way in which we define or describe these activities.

2. By stating that "practically all, if not all, learning involves functional connections in the central nervous system" (p. 55), Professor Cason admits that psychological learning cannot be *completely* relegated to the field of neural connections. Since the 'establishment or strengthening of neural connections' during learning is hypothetical, why introduce neural changes at all? Whether all learning involves the establishment of neural connections is an experimental question to be answered by studying what is happening internally when learning activities occur. Therefore, it would apparently be better to define learning in terms of observable data or conditions, and then to determine what neural changes, if any, are correlated with learning. Cason insists "that the psychological processes which actually occur in the body should be taken into consideration" (p. 61). We will show below that the learning-remembering process can be defined in terms of changes in behavior of living organisms, without recourse to neural changes which have not *yet* been correlated with concomitant learning activities.

[3] *Cf.*, R. S. Woodworth, Psychology (3d ed.), New York: Henry Holt, 1934, p. 29.

3. A reduction of learning to hypothetical and as yet uncharted neural changes does not give that systematic unity which a definition of learning should have. Such a definition should enable one to differentiate between those instances of modifications of behavior which are generally classified as learning activities and those modifications of behavior which are not classed as learning activities, *e.g.*, fatigue, sensory adaptation, increased excitability, disease, etc. Thus, suppose that one of three instances (X, Y, and Z) of modifications of behavior was clearly learned but that the other two were doubtful cases; and suppose that Professor Cason were then asked to apply his definition so as to make the correct differentiation. Could he do so?

4. Even if a neural definition of learning were adequate, note that Professor Cason apologizes for these neural connections by saying: ". . . Including the concept of neural connections in the definition of learning does not mean that the nervous system will have to be thought of or specifically mentioned every time a reference is made to the phenomena of learning" (pp. 55–56). If it is true that neural connections are the underlying bases of learning, why should they not be thought of when we discuss the phenomena of learning? It is difficult for some people to think of heredity or of heat (both examples of Professor Cason's) without thinking of genes and of molecules respectively.

5. Professor Cason states: "The part of our definition following the words 'as a result of' is the *cause*, and the part preceding these words is the *result*" (p. 57). In regard to this statement we would ask: Should a definition of the learning process involve a cause-and-effect *explanation?* More specifically, can Professor Cason demonstrate that 'accompanying or immediately preceding psychological activities' really are the *cause* of an *effect* described as 'the establishment or strengthening of neural connections between stimulating processes and responding processes'? We shall demonstrate below that definitions of learning which are wholly *descriptive* are quite adequate.

6. The word 'immediately' in the definition of learning above "means a fraction of a second or a very few seconds at most" (p. 58). Why limit learning to 'a result of accompanying or *immediately* preceding psychological activities'? If today for the first time in my life a dancing master demonstrates to me how one should stand, hold one's partner, and move one's feet, and I do not engage in the motor practice of any of these activities until tomorrow, are we not justified in saying that the learning process which began today is going on tomorrow?

7. 'Remote or retroactive influences' (p. 58) are ruled out of the concept of learning. This is apparently because Professor Cason takes the word 'retroactive' (in the dictionary sense) to imply acting backward. For example, he points out that if an individual dances on each of the first nine days in January, but receives no instruction in dancing until January 10th, "there is no sense in which the learning of *Jan. 10* can have any kind of 'retroactive' effect on the *dancing activity* of the *first nine days*." With this assertion one must agree, of course, but there is no suggestion in the term 'retroactive' *as employed by psychologists* to imply such an effect. Reasonable men agree that a particular word may be used to mean what one wants it to mean, provided only that the word is always used in a way consistent with that meaning. Retroactive as employed in psychological literature does not mean 'acting back on' past activities. The term implies only an *as if* validity,[4] and continues to be employed only because it has been used so long in the literature and because its definition as a phenomenon does not imply an explanation.[5]

8. Learning and retention are not only treated as if they were entities but are divided as though they were separate sorts of entities. It seems essential to realize that learning and retention (or better, remembering) occur on the same

[4] *Cf.*, H. Vaihinger, Philosophy of as if, 1924, *passim*.

[5] *Cf.*, J. A. McGeoch, On the term 'retroactive inhibition,' *Amer. J. Psychol.*, 1930, 42, 455–457. McGeoch also notes that science employs other terms which are far more metaphorical, *e.g.*, 'refractory phase.' See in this connection, S. H. Britt, Retroactive inhibition: A review of the literature, *Psychol. Bull.*, 1935, 32, 382.

time continuum; that is to say, remembering is in certain respects a temporal continuation of the learning process. Thus:

Learning Remembering

Time →

Learning and remembering can be separated only for purposes of logical analysis. There is such an interrelation and integration of the activities of learning and remembering that it is accurate to speak of the learning-remembering process.

9. Forgetting is also treated by Cason as though another entity were involved. Yet *forgetting* is really only the activity of remembering described from a different point of view. Thus, our diagram may be modified:

Learning Remembering (opposite aspect, Forgetting)

Time →

For Cason to say that retention is the *persistence* of learned connections, and that forgetting is the *loss* of learned connections, does not indicate sufficiently that remembering and forgetting are converse labels for the same type of activity. Fleas may not necessarily *persist* in biting a dog, but that is no indication that the dog has *lost* those fleas: only non-persistence of the fleas is proved.

10. Finally, reproduction is also defined as if there were another special entity, and as though of the same importance as learning, retention, and forgetting. We may well ask in what way reproduction is so unique as not to be subsumed under the learning-remembering (or learning-forgetting) process. How does reproduction differ essentially from the usual measures (recognition, recall, relearning) employed to determine quantitative relationships of the learning-remembering process? Our diagram should really look like this:

Learning Remembering (opposite aspect, Forgetting)
—measured in terms of Recognition-Recall-Relearning (Remastery)

Time →

## II

Since our criticism has been partly destructive, we shall suggest what seem to us more adequate definitions:[6]

*Learning is the acquisition by a living organism of a mode of response which is adapted to a motivating problematical situation.*

*Remembering is the retention by a living organism of a previously acquired mode of response which is adapted to a motivating problematical situation.*

*Forgetting is the non-retention by a living organism of a previously acquired mode of response which is adapted to a motivating problematical situation.*

(Therefore) *The learning-remembering (or learning-forgetting) process is the acquisition and retention (or non-retention) by a living organism of a mode of response which is adapted to a motivating problematical situation.* The learning-remembering (or learning-forgetting) process may be measured in terms of recognition, recall, and relearning (remastery).

To repeat the first definition, *learning* is: (1) the acquisition (2) by a living organism (3) of a mode of response (4) which is adapted (5) to a motivating (6) problematical situation.

(1) 'The acquisition': There must be development, *i.e.*, progressive change in behavior.

(2) 'By a living organism': There is no learning in inorganic matter. However, the concept of learning is not restricted so as to apply only to those living organisms which

[6] As examples of the possibility of defining learning without specific reference to demonstrable neural connections, both Professor Hunter and Professor Carr have offered definitions superior to Professor Cason's. Hunter, a behaviorist (or 'anthroponomist') says: "In general . . . we may say that learning is taking place wherever behavior shows a *progressive change*, or *trend*, with a repetition of the same stimulating situation and where the change cannot be accounted for on the basis of fatigue or of receptor and effector changes. Those trends in behavior which thus earn the title of learning are *presumably* (italics ours) due to changes in the nervous system and are possible because of the plasticity and retentivity of this system" (W. S. Hunter, Ch. 11, *Experimental Studies of Learning*, in 'A Handbook of General Experimental Psychology,' 1934, p. 497). Carr, a functionalist, says: "Learning may be defined as a process of modifying or reorganizing our customary modes of procedure in such a way as to enable us to react successfully to novel situations" (H. A. Carr, Psychology, 1925, p. 101).

are multicellular and which possess a nervous system. The term applies equally well to all Protozoa and Metazoa.[7]

(3) 'Of a mode of response': The establishment of a change in behavior is necessary, although this change need not necessarily involve overt movement. Some modification of the organismic pattern is what is implied.

(4) 'Which is adapted': There must be adjustment to the situation.

(5) 'To a motivating': There must be a stimulus or stimuli sufficiently persistent to influence the behavior of the organism to the extent that the behavior is no longer affected in the same way by the same stimulus or stimuli.

(6) 'Problematical situation': The organism must be confronted by a problem; that is to say, there must be some lack of adjustment between the organism's (*a*) motivating needs, (*b*) its reactive equipment, and (*c*) the sensory situation.[8] Thus, in phenomena such as sensory adaptation and fatigue, there are modifications in the behavior of the organism which are not classed as learning.

The learning process means that the organism is potentially capable of continuing to modify its behavior, but not that it will necessarily do so. If the organism does so continue to modify its behavior to a motivating problematical situation, remembering is involved; that is, the organism has shown a capacity to change its behavior in accordance with past situations or past reactions. If, with repetitions of the same or similar situations, the adjustment between the organism's motivating needs, its reactive equipment, and the sensory situation becomes simpler, more rapid, and more enduring, we have evidence that remembering is occurring.

The same type of analysis may be applied to the other three definitions suggested.

The ten objections to Professor Cason's analysis are not involved in these definitions. (1) The learning-remembering

[7] *Cf.*, H. S. Jennings, Behavior of the lower organisms, New York: Columbia Univ. Press, 1915; esp. Chap. XIII, 'Comparison of Behavior of Unicellular and Multicellular Organisms.'

[8] *Cf.*, Carr, *op. cit.*, p. 87.

process is not reified. (2) Uncharted neural connections are not posited. (3) A systematic unity of learning activities, as differentiated from non-learning activities, is not neglected. (4) No apology for neural connections is necessary. (5) A cause-and-effect explanation is not presented, but only a description of the learning-remembering process. (6) The process is not limited to the influence of immediately preceding activities. (7) A disregard of 'retroactive' (in the psychological sense) influences is not implied. (8) The processes of learning-remembering are not artificially separated. (9) That forgetting is simply the opposite aspect of the remembering process is not disregarded. (10) Neither reproduction nor other measures of the learning-remembering process are treated as of the same class as learning-remembering-forgetting.

[MS. received February 24, 1937]

# THE ORGANIZATION OF BEHAVIOR IN THE NEWBORN INFANT[1]

BY KARL C. PRATT

*Central State Teachers College*

## Introduction

*The classification of neonate behavior.*[2]—The general literature upon the newborn infant belongs, in the main, to the classificatory phase of the natural sciences. Writers upon the neonate seek to discover, to explore and to catalogue the many items of behavior, and to make an inventory or complete list of the discrete responses in the infant's behavior repertoire. Items in this repertoire are classified in terms of now this and now that set of categories but, with the possible exception of Furfey, Bonham and Sargent's study (14), rarely do those who classify or report neonate behavior successfully envisage or reveal the organization of behavior which is characteristic of this period of human ontogenesis. A survey of the possible bases of organization shows that the classification of neonate reflexes or responses may be in terms of the stimuli or sense organs involved in the behavior under study, in terms of anatomical topography, or in terms of the parts of the organism which participate in the response. Again, a classification may represent an attempt to separate responses into those which are inborn and those which are learned. Or it may reveal an attempt to deal with the various behavior items from some teleological approach. None of these types of classification can provide either the cross-sectional data which are necessary for a measure of the organization of behavior at a given life-history period of the organism, or

[1] The principal features of this article were embodied in a paper read at the meetings of the American Psychological Association at Ann Arbor in 1935 (see 34).

[2] The problems peculiar to the different systems of classification of neonate behavior have been treated in detail elsewhere (35).

data essential for an understanding of the genetic sequences in successive cross-sections.

*The nature of 'specific' and of 'generalized' behavior.*—In recent papers (**30**, **35**) the writer has attempted to establish the basis for a quantitative analysis of neonate behavior in terms of intervals along degrees in a generalization-specificity scale. To facilitate the development of such a scale he has advocated first, a refinement in definition, and second, an extension of the generalized-to-specific concept of development which is explicit in the work of Minkowski, Bersot, and Coghill. This concept would be extended and broadened to include stimulus-receptor-effector relations not only in the longitudinal progression but also in the cross-sectional organization of the newborn organism at numerous stages. It has seemed to the writer that the term 'specificity' refers to a relative limitation in the mechanisms of stimulus and response, whether longitudinally or cross-sectionally viewed. The employment of this term suggests that in the genetic sequences of a given response one might expect to observe changes in the number of stimulus modalities which are involved in given activities, in the number of sense organs involved, and the extent of effector-segment units which are involved in various responses of the organism. The observations of Coghill (**8**, **9**) upon inframammalian forms, of Minkowski (**22**) in human fetal development, of Bersot (**3**) in the pre- and post-natal human stages, Pratt, Nelson and Sun (**36**) and of Irwin (**17**) in the neonate period seemed to indicate that, *with certain exceptions*, both unlearned and learned behavior progressed from generalized to specific phases. The discovered progression in behavior is apparently of such nature that any one phase of a response can be designated as specific or generalized in relation to preceding or to subsequent phases. The possibility of making such a comparison of *different* responses at any given age level and of demonstrating its utility in different physiological states of the organism has appeared feasible.

The usefulness of the generalization-specificity concept in describing the organization and changes in the organization

of an organism's behavior is not inseparably linked with any particular formulation of the principles of development, but this concept should prove to be a valid one in any attempt to evaluate such an organization. In his own endeavor to clarify the use of the terms 'specificity' and 'generalization,' the writer has considered the latter term as applying most appropriately (in the sense of the maximal) to a stage of development in which a stimulus will involve the organism most extensively. In man this stage is not the initial period of development but rather falls from the middle fetal stage on through the neonatal stages. This view coincides to that extent with Carmichael's (6) view of the problem. We can only speculate as to the reasons why some psychologists have formulated a law of development to the effect that, *from the beginning*, the sequence is from the generalized to the specific. Possibly that conclusion has been encouraged by the fact that many of the early responses of an organism are *massive* and involve the large muscle groups which, when in action, result in the passive or *mechanical* movement of numerous smaller parts of the organism. From one point of view this would constitute a more general response; nevertheless it seems preferable to reserve the term 'generalization' for a state characterized by *physiological* or active participation of the parts.

*Pre-natal behavior development and the structural mechanisms as determined by Minkowski.*—Although the inventory which is to be made in this paper will deal primarily with the organization of behavior on the neonate level, the basic features of prenatal development will also be reviewed briefly in order to indicate the applicability of the quantitative treatment to behavior in the longitudinal plane.

In the prenatal interval of human existence, according to Minkowski (22, 23), there arises a stage of aneural or idiomuscular activity. Minkowski holds that in the embryonic period this condition prevails until about the second month of prenatal life.[3] Angulo y Gonzalez (1, 2) and Angulo y

[3] As Carmichael (7) has emphasized recently this is hypothetical because Minkowski observed fetuses which already had functional neural elements. Minkowski justifies

Gonzalez and Coghill (10) have demonstrated the existence of this state of reactivity in the early phases of development in the albino rat. In short, they claim that the earliest localized movements which can be elicited cannot be reflexes, as Windle reports, because the nervous system is not yet functional. This must be the case, since the responses can be elicited even when the embryo is curarized. However, Bridgman and Carmichael (4), in their experiments upon the fetal guinea-pig, state that some of the earliest fetal responses are quite specific and are of neurogenic rather than myogenic origin. It would seem desirable that crucial experimentation be undertaken to determine this very important point.

The question arises as to whether such behavior, if myogenic, may be accurately termed 'organismic' since at this early stage no definite means of physiological correlation would appear to exist. Perhaps this early type of reactivity is better designated 'tissue' behavior, because it displays characteristics which mark the receptor and neural functions which are the intrinsic property of muscle tissue itself. How shall this type of activity be classified? Shall the term 'specific' be applied, or reserved for that organismic behavior which involves the neural propagation of energy from irritated receptor tissues to effectors which are thereby aroused? Myogenic activity is limited or localized with respect to the elements stimulated because of the lack of differentiation or of specialization in the above-mentioned structures. The truly individuated or discrete response when it appears later is organismic in character since it is accomplished through the participation of differentiated structures. In other words, when restriction of response does appear it is accomplished largely through correlational mechanisms rather than through the limited involvement of one tissue alone. The earlier 'tissue' response does present a certain amount of localization, but mere localization is not the sole criterion of specificity in a response. Indeed it may be well to term 'tissue' behavior a

his hypothesis from experiments in which nerves were sectioned with continued persistence of what he termed idiomuscular responses. He also draws upon direct studies of this earliest fetal period of reactivity in lower animals.

'reaction' and reserve the term 'response' for true organismic activity.

Following the period of purely myogenic or idio-muscular (Minkowski) activity there appears at about two months a transitional stage during which activity may either be idio-muscular or released through neural innervation. At that stage movements are attributable principally to the reactivity of the muscles themselves and in character are particularly slow, asymmetrical and arythmical; but the generalization and variability of certain responses are evidence that neural factors have become involved (in this case, spinal integration is possible).

From the third to the fourth month, or in the early fetal stage, we find, according to Minkowski (**23**), the generalized features which characterize all fetal behavior.[4] This stage also manifests the beginnings of specific movements which are becoming disengaged from the more generalized responses and are limited according to the nature and the site of the excitations.[5] Type examples of these 'short' reflexes, according to Minkowski (**23**) are the reflexes of flexion and extension of the lower extremity. The flexor phase is that of triple retraction at hip, knee and ankle. As we have noted elsewhere (**30, 32**) a response such as this is specific in the sense of segmental participation only in comparison with a more generalized state, and is generalized in comparison with still more segmentally limited responses.

In the intermediate fetal stage (4th–6th months) supra-spinal elements are first involved in behavior. The period

[4] "D'une manière générale, on peut dire que, à cette période, chaque segment des téguments peut constituer une zône réflexogène pour des réactions très diverses, qui ont la tendance à s'étendre à des parties plus ou moins étendues de l'organisme foetal, c'est-à-dire *à irradier et à se généraliser*, comme cela caractérise en général les réactions foetales" (p. 6).

[5] ". . . en excitant la peau d'une extremité, on voit, avec l'âge, un mouvement particulier de celle-ci se dégager de plus en plus de la 'figure' réactionnelle diffuse des stades plus précoces, dans laquelle un mouvement réflexe de l'extrémité excitée était inclus, mais ne ressortait pas spécialement. C'est alors que, successivement, apparaissent des *réflexes* 'courts,' qui paraissent être limités complètement ou du moins essentiellement à l'extrémité excitée bien que l'irradiation du processus d'excitation dépasse de beaucoup l'effet locomoteur manifeste" (p. 7).

is characterized by a further restriction or localization of reactions at the same time that particular co-ordinations which give definite response patterns ('figures réactionnelles') are being established. In addition to the 'short' reflexes (homolateral) there appear in this period 'crossed' reflexes (contra-lateral), 'long' reflexes (homolateral—spreading from anterior to posterior or vice versa), and diagonal reflexes.

From the foregoing account it seems clear that as the nervous system matures more of an organism may be involved upon excitation of a given area. The hooking-up of localized part activities into a larger pattern of reaction may thus be brought about by means of the maturation of the nervous system. From the standpoint of actual *organismic involvement* or of *segmental participation* the response thus becomes more generalized. In terms of the comparative regularity of occurrence it may possess one of the characteristics of specificity (**30**).

In the late fetal or the sub-cortical stage (6th–9th months) there is a continuation of the developmental features which are observable in the preceding period.

Cortical participation in the control of responses begins in the neonatal period. Movements in this stage and in the early suckling age are choreic, athetosic and spasmodic. According to Bersot (**3**) a maximal generalization of the plantar response is found at about two months after birth.

From the foregoing summary or paraphrase of Minkowski's findings it would seem that although specific responses do appear at different points in the developmental series, the general impression which the observer obtains is one of the generalization of behavior in the late fetal and in the neonatal levels of development. As different portions of the nervous system mature successively the possibilities of greater organismic involvement are increased. At these two levels of development almost all receptors become linked up with almost all effectors. Hence the stimulation of virtually almost any receptor or receptor group *may* release activity in virtually any of the effector segments of the body. The

wide-spread integration which is thus displayed is accomplished through the nervous system. In practically all accounts of neonate behavior the terms 'generalization of response' and 'irradiation of neural impulses' have applied to this phase.

Under certain physiological and developmental conditions particular stimuli may evoke responses which are comparatively limited with respect to time or to the extent of organismic involvement. Such responses are usually spoken of as 'specific' responses. These more or less individuated responses are to be thought of as emerging from a *stage* in which generalized behavior predominates rather than from some particular instance of *continuous* or *sustained* generalized behavior.

In short, when development attains a certain stage certain types of stimuli, either exogenous (*e.g.*, thermal, 'pain,' etc.) or endogenous (*e.g.*, stimuli from the alimentary tract or from other visceral tissues) [6] release a prolonged, continuous or repetitive generalized response. This condition of activity has been variously termed 'random,' 'spontaneous,' or 'mass' activity. Obviously, these terms are all misleading, for the activity in question is neither completely unorganized nor is it without certain observable regular sequences; it does not occur spontaneously without necessary attendant conditions, nor is it the behavior of an undifferentiated mass.[7]

Other types of stimuli (and in particular those which are discontinuous) may release quite localized responses, or on the other hand they may release a generalized response which runs its course and is then followed by quiescence.

[6] The earlier investigators of 'hunger' in animals, human adults, and infants linked the general activity with muscular contractions of a particular type in the stomach, and ascribed a causal relationship. Irwin (18) demonstrated a positive correlation between 'mass' activity and time elapsing after the nursing period. Richards (37, 38, 39) has denied that the stimuli for general bodily activity are provided by the muscular contractions of the stomach although distention of that organ tends to inhibit the bodily activity. He postulates the arousal of gastric activity as a specific reaction and of bodily activity as a general reaction to the general nutritional state of the organism.

[7] For a searching criticism of the attempt made by some writers to identify this "mass action" as the initial stage of behavior, the reader may consult Carmichael's recent monograph (6).

## An Inventory of Behavior Organization in the Neonate

*A technique for the making of an inventory.*—As has been noted previously, most classifications of neonate behavior have not been designed to give insight into the *organization* of such behavior. In general, the studies either present lists of responses at given developmental levels or pursue the sequential development of certain important activities such as postural adjustments, locomotion, manipulation, and the like. Such studies have an evident practical value, yet it has become a truism that a human being is not to be represented as a sum-total of part activities. An organismic picture can be obtained only when the treatment embodies a portrayal of the organization or interrelation of the activities of an organism.

In order to trace the development of organismic behavior a type of inventory is required which will account for the various types of responses, and which will have reference not only to the longitudinal progression of particular response patterns but which will also present the interrelations among responses at any given cross-sectional level.

What are the lines to be followed in the preparation of an inventory which shall comply with these requirements? To offer a tentative answer, the basis for an inventory of behavior organization in the newborn infant, as well as behavior organization at preceding and at subsequent stages, will be afforded by:

(1) Ascertaining the types of stimuli which effectively act upon the receptors. At the neonate level, and indeed at later periods as well, it is not yet possible to state with any great precision or inclusiveness for each receptor or group of receptors the effective modes of energy and their ranges. When studies of the relative specialization of receptor tissue have been made, it will then be possible to show that those receptor groups which are activated by the most modes of energy are the most generalized, and similarly that the more extensive the range of the effective energy the more generalized is the receptor.

(2) A tabulation of the number of response patterns which may be elicited through single stimulus-receptor channels (Table 1). This approach relates the better known response patterns, as described by Canestrini (5), Peiper (24, 25, 27), Irwin (17, 19), Richards and Irwin (39, 40), Dennis (12) and others, to the stimulus-receptor agencies by which they are released. It is to be noted that in the present inventory the approach to a study of stimuli is by way of a sense-organ or receptor viewpoint rather than through a classification of stimuli on a physical basis. This somewhat artificial practice is necessitated by our ignorance of the effect of different types of energy changes upon particular receptors and by our inability to determine which receptors are being affected in given cases. In short, at present we are not prepared to pass upon the 'selectivity' of receptors. However, it can be safely concluded that the more responses which are elicitable through a given stimulus-receptor channel the more generalized is the channel.

(3) Totalling the number of stimulus-receptor agencies which will release a given response pattern. The degree of specificity of a given stimulus-receptor agency is shown according to whether it obtains unique possession of the response pattern or shares it with other receptor groups.

(4) Making a segmental or factorial analysis of response patterns to determine the relative motor involvement of the organism in each case (Table 2). In this instance the greater the number of segmental elements participating in a given response, the more it may be said to be generalized.

The procedures outlined above should provide a tentative assay of the organization of behavior at the neonate level, and should enable one to follow the development of this organization through its passage into other levels.

*The inventory itself.*—In Table 1 are presented the stimulus-receptor agencies releasing the better known response-patterns in the newborn infant. It is quite apparent that more responses are released by pressure or tactual stimuli than by any other means. The generalized aspect is here accounted for by the widespread distribution (especially in

## TABLE 1

STIMULUS-RECEPTOR AGENCIES RELEASING RESPONSES

| Responses | Types of agencies | | | | | | | | | No. of types |
|---|---|---|---|---|---|---|---|---|---|---|
| | P | T | SP | V | I | N | A | G | O | |
| 1. Circulatory | # | # | ? | # | # | # | # | # | # | 8–9 |
| 2. Respiratory | # | # | ? | # | # | # | # | # | # | 8–9 |
| 3. Inhibitory | # | # | # | # | # | | # | # | | 7–7 |
| 4. Crying | ? | # | | | # | # | # | ? | ? | 4–7 |
| 5. Head-back | #1 | #1 | | # | | #1 | | | ? | 4–5 |
| 6. Sucking | #1 | | | | # | | | # | # | 4–4 |
| 7. General (mass activity) | ? | # | | | # | # | ? | ? | ? | 3–7 |
| 8. Palpebral | #1 | | ? | # | | | # | ? | ? | 3–6 |
| 9. Moro | ? | ? | # | # | | | # | | | 3–5 |
| 10. Facial | | #1 | | | # | # | | ? | ? | 3–5 |
| 11. Pupillary | # | | | # | | # | ? | | | 3–4 |
| 12. Nystagmus | | #1 | # | # | | | | | | 3–3 |
| 13. Plantar | #1 | | # | | | | # | | | 3–3 |
| 14. Leg withdrawal | #1 | #1 | | | | #1 | | | | 3–3 |
| 15. Body jerk (startle) | ? | ? | | # | ? | ? | # | ? | | 2–7 |
| 16. Arching back | ? | #1 | | | | #1 | | | | 2–3 |
| 17. Palmar | #1 | | # | | | | | | | 2–2 |
| 18. Creeping-phenomenon | #1 | | # | | | | | | | 2–2 |
| 19. Stepping movements | #1 | | # | | | | | | | 2–2 |
| 20. Defecation | | ? | | | # | ? | ? | | | 1–4 |
| 21. Micturition | | ? | | | # | ? | ? | | | 1–4 |
| 22. Regurgitation | | ? | | | # | | | | | 1–2 |
| 23. Mouth rejection | | | | | | #1 | | ? | | 1–2 |
| 24. Shivering | | # | | | ? | | | | | 1–2 |
| 25. Head-mouth orientation | #1 | | | | ? | | | | | 1–2 |
| 26. Head-eyes orientation | | | | # | | | | | | 1–1 |
| 27. Head orientation to rotation | | | # | | | | | | | 1–1 |
| 28. Eye-pursuit | | | | # | | | | | | 1–1 |
| 29. Fixation | | | | # | | | | | | 1–1 |
| 30. Gasping | | # | | | | | | | | 1–1 |
| 31. Spinal and abdominal | #1 | | | | | | | | | 1–1 |
| 32. Hiccuping | | | | | # | | | | | 1–1 |
| 33. Swallowing | #1 | | | | | | | | | 1–1 |
| 34. Salivary | | | | | | | | # | | 1–1 |
| 35. Lip reflex | #1 | | | | | | | | | 1–1 |
| 36. Patellar | #1 | | | | | | | | | 1–1 |
| 37. Brudzinski | | | # | | | | | | | 1–1 |
| 38. Tonic-neck upon body | | | # | | | | | | | 1–1 |
| 39. Tonic-neck reflex upon limbs | | | # | | | | | | | 1–1 |
| 40. Balancing | | | # | | | | | | | 1–1 |
| Minimum number | 17 | 12 | 12 | 12 | 11 | 10 | 8 | 5 | 3 | |
| Maximum number | 22 | 17 | 15 | 12 | 14 | 13 | 12 | 11 | 8 | |
| Per cent of responses (minimum) | 42 | 30 | 30 | 30 | 27 | 25 | 20 | 12 | 7 | |
| Per cent unique responses | 24 | 8 | 42 | 25 | 9 | 0 | 0 | 20 | 0 | |

Legend:

P, pressure or contact — T, thermal — SP, static and postural
V, visual — I, internal — G, gustatory
N, noxious or pain — A, auditory — ?, probable
O, olfactory — #, effective
#1, effective through local area

the cutaneous surfaces or areas) of receptors affected by this particular type of stimulus rather than by the elicitation of a great number of responses through the excitation of any given receptor area (31). The well-known effects of a pressure upon the eyeball which is transmitted to the visual receptors afford a suggestion that this type of stimulation may not be entirely ineffective when it acts upon highly limited and specialized sense organs. The difficulty of separating the effects of thermal or of gustatory stimulation from those due to contact or pressure or noxious stimuli also indicates the wide-spread possibility of response by the organism to this type of change in the environment. On the other hand, the fewest responses are released by the gustatory and by olfactory modalities, the smallest relative number by olfactory if stimuli affecting other than specific olfactory receptors are ruled out.[8] If the extent of distribution of receptors is considered (the present emphasis being upon receptor rather than upon nature of stimulus) it seems that more responses are released per receptor area by visual stimuli than by any other type. The static-postural category also ranks high, but in the case of many responses aroused through these receptors it is uncertain whether static or kinesthetic receptors, or both, are involved.

The percentage of responses which are unique to the respective individual stimulus-receptor agencies reveals a complete lack of specificity among responses given to noxious, auditory and olfactory stimuli, and an almost complete lack in the case of thermal and internal stimuli. Static and postural stimuli provide the greatest degree of such specificity with 42 per cent unique responses, followed by visual with 25 per cent, pressure or contact with 24 per cent and gustatory with 20 per cent.

On these grounds it may safely be affirmed that from the stimulus-receptor aspect there is no evidence for a high degree of specificity of behavior in the newborn infant.

[8] Recent studies by Stirnimann (42, 43) in indicating a wealth of differential reactions, particularly mimetic, run counter to the work of Pratt, Nelson and Sun (36) and Jensen (20).

Viewed from the angle of the number of stimulus-receptor agencies effective for a given response one observes a gradation from a maximal *generalization* in the case of the circulatory, respiratory and possibly inhibitory responses, on through to a maximal *specificity* in hiccuping, in gasping, in Thompson's lip reflex, and in others. In all, 14 responses, or 35 per cent of the 40 listed in Table 1, may be released by at least 3 of the 9 stimulus-receptor agencies; 19 (47 per cent) and possibly 25 (62 per cent) may be released by at least 2 different stimulus-receptor agencies; while 15 (37 per cent) may each be released through one stimulus-receptor channel only.

In Table 2 is presented an analysis of 21 response patterns in terms of segmental participation. The major segments of the body are taken to be the head, trunk, arms and legs with the bilateral representation of the last two ignored. Within each major segment, smaller segments or action units are represented. The latter are by no means exhaustively treated here but are probably sufficiently represented for preliminary sampling purposes. Circulatory and respiratory responses are represented as having their site in the trunk, although obviously the former must be virtually organismic in character. Along these lines, 7 or 33 per cent of the 21 responses involve all major segments of the organism, while 19 or 90 per cent involve at least 2 of the major segments. Considering all behavior segments or action units represented in this table, the range is from maximum generalization of organismic involvement or segmental participation in the case of "mass" or general activity to least segmental participation in the case of activities such as hiccuping. Ultimately our comparison of responses upon a factorial basis must include a statement of the respective stimulating conditions, attention being directed particularly to intensity and temporal aspects of the stimuli. At present, taking auditory stimuli as our example, we must emphasize that the extent of organismic involvement, whether in the case of continuous or of discontinuous stimulation depends upon the intensity of the stimulus and the present state of the organism. Thus the presentation of a brief, moderately intense auditory stimulus

## TABLE 2

### SEGMENTAL PARTICIPATION IN RESPONSES

| Segments | Responses | | | | | | | | | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | General | Moro | Mouth rel. | Inhibitory | Head-back | Creep | Startle | Plantar | Stepping | Tonic-neck | Suck & Swal | Head-mouth | Gasping | Spinal | Brudzinski | O. c. gyric | Nystagmus | Palpebral | Balancing | Tonic-neck* | Hiccuping | Minimum N | Maximum N | % Minimum |
| *Head* | ## | ## | ## | ## | ## | ## | ## | | | | ~## | ## | ## | | | ## | ## | ## | ## | | | 11 | 14 | 52 |
| Upon body axis | x | | x | x | x | x | x | | | | | x | | | | x | x | | | | | 10 | 11 | |
| Rotation | x | | x | x | | | | | | | | x | | | | ~ | | | | | | 4 | 5 | |
| Eyes | | | | | | | | | | | | | | | | | | | | | | | | |
| Lids | x | x | | | ? | | x | | | | | | | | | | | x | | | | 4 | 5 | |
| Eyeballs | x | | | | | | | | | | | | | | | x | x | | | | | 3 | 3 | |
| Iris | | ? | | | x | | ? | | | | | | | | | | | ? | | | | 1 | 4 | |
| Face | | | | | | | | | | | | | | | | | | | | | | | | |
| Facial muscles | x | ? | x | | x | | | | | | | x | | | | | | | | | | 4 | 5 | |
| Mouth | | | | | | | | | | | | | | | | | | | | | | | | |
| Lips | x | ? | x | | | | | | | | x | x | x | | | | | | | | | 5 | 6 | |
| Tongue | x | ? | x | | | | | | | | x | | | | | | | | | | | 3 | 4 | |
| Jaw | x | ? | x | | | | | | | | x | x | x | | | | | | | | | 5 | 6 | |
| Salivary glands | | | | | | | | | | | x | | | | | | | | | | | 1 | 1 | |
| *Arms* | ## | ## | ## | ## | ## | ## | ~## | ~## | ~## | ## | | | | | | | | | | | | 10 | 10 | 48 |
| Upper | x | x | x | x | x | x | ~ | ~ | ~ | x | | | | | | | | | | | | 7 | 10 | |
| Fore | x | x | x | x | x | x | ~ | ~ | | x | | | | | | | | | | | | 7 | 10 | |
| Hand | x | x | | ? | | x | | ~ | | ~ | | | | | | | | | | | | 3 | 6 | |
| Fingers | x | x | | ? | | | | ? | | ? | | | | | | | | | | | | 2 | 5 | |

TABLE 2—*Continued*

| Segments | Responses | | | | | | | | | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | General | Moro | Mouth rej. | Inhibitory | Head-back | Creep | Startle | Plantar | Stepping | Tonic-neck | Suck & Swal | Head-mouth | Gasping | Spinal | Brudzinski | O. c. gyric | Nystagmus | Palpebral | Balancing | Tonic-neck* | Hiccuping | Minimum N | Maximum N | % Minimum |
| *Trunk* | # | # | # | # | # | # | # | # | # | # | # | # | # | # | # | # | # | # | # | # | # | 12 | 21 | 57 |
| Upon body axis | x | x | x | x | x | x | x | ? | ? | | | | | x | | | | | | | | 8 | 11 | |
| Rotation | x | | x | x | | x | | ? | ? | | | | | | | | | | | x | | 5 | 7 | |
| Respir.—diaphragm | x | x | ? | x | x | ? | x | ? | ? | ? | x | ? | x | ? | ? | ? | ? | ? | ? | ? | x | 8 | 21 | |
| —other | x | x | ? | x | x | ? | x | ? | ? | ? | x | ? | x | ? | ? | ? | ? | ? | ? | ? | ? | 7 | 21 | |
| Circulatory | x | x | ? | x | x | ? | x | ? | ? | ? | x | ? | x | ? | ? | ? | ? | ? | ? | ? | ? | 7 | 21 | |
| *Legs* | # | # | # | # | # | # | # | # | # | # | | | | # | # | # | | | | | | 12 | 12 | 57 |
| Thigh | x | x | x | x | x | x | ? | x | x | x | | | | x | x | | | | | | | 11 | 12 | |
| Leg | x | x | x | x | x | x | ? | x | x | x | | | | x | x | | | | | | | 11 | 12 | |
| Foot | x | x | | ? | ? | x | | x | x | ? | | | | | x | | | | | | | 6 | 9 | 8 |
| Toes | x | x | | ? | ? | x | | x | x | ? | | | | | | | | | | | | 5 | 8 | |
| Major body segments | 4 | 4 | 4 | 4 | 4 | 4 | 4 | 3 | 3 | 3 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 1 | 1 | | | |
| Minimum participation | 21 | 13 | 12 | 11 | 11 | 10 | 6 | 4 | 4 | 4 | 7 | 5 | 5 | 3 | 3 | 2 | 2 | 1 | 1 | 1 | 1 | | | |
| Maximum participation | 21 | 18 | 15 | 15 | 14 | 13 | 11 | 13 | 11 | 11 | 8 | 8 | 5 | 6 | 6 | 2 | 6 | 5 | 4 | 4 | 3 | | | |

Legend:
O. c. gyric, oculocephalogyric
#, participation of, or within segment
x, participation of segments
*, tonic-neck upon body

tends to release the Moro response which is highly generalized; whereas repetition of the stimulus or the initial presentation of a weaker stimulus may release a response with reduced segmental involvement, the startle. On the other hand, there may remain only palpebral responses accompanied by respiratory and circulatory changes (**28**, **29**). In passing, it may be noted in this connection that the Moro reflex, at least as far as its elicitation through jarring is concerned, is supposed to disappear in early infancy. Dennis (**13**) has criticized this inference as to early disappearance of the Moro reflex, on the ground that in older children the test situation tends to inhibit the response. Further, in view of the fact that the 'startle,' which from Hunt and Landis' description (**15**, **16**) is identifiable as the Moro response, can be elicited in adults by intense auditory stimuli, one is impressed with the necessity of knowing the intensity of the stimulus as well as the age of the organism before attempting to pass with any finality upon the items in the behavior repertoire of an organism.

Viewing only the minimal major-segment representation in the 21 responses included in Table 2, it is apparent that no one major segment is involved appreciably more than are others.

In Table 3 are combined the number of effective stimulus-receptor agencies (Table 1) and the number of segmental elements participating in the responses analyzed in Table 2. Such a treatment enables one to make a simultaneous comparison of the degree of specificity in each. In general it may be said that there is a tendency for responses with the most extensive segmental participation to be released by the greatest number of stimulus-receptor agencies. In other words, the degree of generalization of stimulus-receptor agencies is correlated with the degree of segmental participation in the response. The order of the responses in this table, as was the case for Table 2, is from those responses with the most to those with the least participation of major body segments such as head, arms, trunk, and legs.

## TABLE 3

SPECIFICITY TABLE

| Responses | N S-R agencies | | Per cent of total | | N of segments | | Per cent of total | | Composite index | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Min. | Max. | Min. | Max. | Min. | Max. | Min. | Max. | Min. | Max. |
| General | 3 | 7 | 33 | 78 | 21 | 21 | 91 | 91 | 24 | 28 |
| Moro | 3 | 5 | 33 | 56 | 13 | 18 | 57 | 78 | 16 | 23 |
| Mouth rej. | 1 | 2 | 11 | 22 | 12 | 15 | 52 | 65 | 13 | 17 |
| Inhibitory | 7 | 7 | 78 | 78 | 11 | 15 | 48 | 65 | 18 | 22 |
| Head-back | 2 | 3 | 22 | 33 | 11 | 14 | 48 | 61 | 13 | 17 |
| Creep | 2 | 2 | 22 | 22 | 10 | 13 | 44 | 57 | 12 | 15 |
| Startle | 2 | 7 | 22 | 78 | 6 | 11 | 26 | 48 | 8 | 18 |
| Plantar | 3 | 3 | 33 | 33 | 4 | 13 | 17 | 57 | 7 | 16 |
| Stepping | 2 | 2 | 22 | 22 | 4 | 11 | 17 | 48 | 6 | 13 |
| Tonic-neck | 1 | 1 | 11 | 11 | 4 | 11 | 17 | 48 | 5 | 12 |
| Suck and Swal. | 4 | 4 | 44 | 44 | 7 | 8 | 30 | 35 | 11 | 12 |
| Head-mouth | 1 | 2 | 11 | 22 | 5 | 8 | 22 | 35 | 6 | 10 |
| Gasping | 1 | 1 | 11 | 11 | 5 | 5 | 22 | 22 | 6 | 6 |
| Spinal | 1 | 1 | 11 | 11 | 3 | 6 | 13 | 26 | 4 | 7 |
| Brudzinski | 1 | 1 | 11 | 11 | 3 | 6 | 13 | 26 | 3 | 6 |
| O. c. gyric | 1 | 1 | 11 | 11 | 2 | 6 | 9 | 26 | 3 | 7 |
| Nystagmus | 2 | 3 | 22 | 33 | 2 | 6 | 9 | 26 | 4 | 9 |
| Palpebral | 3 | 6 | 33 | 67 | 1 | 5 | 4 | 22 | 4 | 11 |
| Balancing | 1 | 1 | 11 | 11 | 1 | 4 | 4 | 17 | 2 | 5 |
| Tonic-neck upon body | 1 | 1 | 11 | 11 | 1 | 4 | 4 | 17 | 2 | 5 |
| Hiccuping | 1 | 1 | 11 | 11 | 1 | 3 | 4 | 13 | 2 | 4 |

Legend:

S-R agencies, stimulus-receptor agencies
Min., minimum
Max., maximum

## DISCUSSION

The writer cherishes no belief that the schema presented in this paper provide a final, adequate, or exclusive blueprint of the organization of behavior in the neonate. On the one hand we lack satisfactory knowledge as to the relation of stimulating conditions (particularly as to stimulus duration, intensity, type, etc.) to the type and extent of the aroused responses and to the influence of important conditions such as age, metabolic and nutritional factors, and antecedent stimulation. The number of studies in which the stimulating conditions have been well controlled and measured, and in which the responses have been adequately observed or recorded, is significantly small. Furthermore very little

progress has been made in standardizing our control of the physiological state of the organism for purposes of more exact experimentation (33).

For the reasons just advanced, the placing of the various response patterns in tables such as these must change as we learn more about the activities themselves. Responses which are now known to be elicited by relatively limited stimulus-receptor agencies may later be found to occur when other agencies become involved under the appropriate stimulating or physiological conditions, and responses which are now considered highly generalized on the stimulus-receptor side may actually be the result of relatively greater stimulus intensities than is the case for the more specific responses. The same statement may be made as to the extent and degree of effector-segment involvement in given responses.

On the other hand, in order to avoid complication, the present tables were not designed to afford any indication of the temporal order or sequence of movements in a response pattern. These features, even in the so-called innate behavior, may, as Delman (11) has reported, account for important differences among response patterns, and hence may be even more significant when learned behavior is under consideration. The present study, directed as it is toward an organization of neonate behavior, also disregards the adaptive integration of different responses in the furtherance of various fundamental activities. The latter approach has been epitomized by the writer (28) but certainly requires more detailed consideration.

In conclusion, it may be said that in the development of the organism the trend seems to be from the undifferentiated or unspecialized to the differentiated and specialized. This statement apparently holds ontogenetically both for learned and for unlearned behavior, and also appears to have phylogenetic significance. In a sense, organismic behavior tends to reduce toward minimal action both in the life-history of the organism and in the history of any particular activity in the organism. This feature has given rise to the formulation of the 'law of least action' (45), which in effect is a description of

what occurs and not a causal analysis. In human beings, the substitution of a verbal response for other types of adjustment is a conspicuous example of this fact, the individual and social significance of which was well understood and ably elucidated by Weiss (**44**). Nevertheless, the number of associated movements (*Mitbewegungen*) as reported by Peiper (**26**) indicate the persistence of generalized responses.

It is also obvious that an organism which has the possibility of *altering* the specificity of its stimulus-receptor-effector relations may attain greater flexibility in meeting environmental changes than is possible in an organism with fixed specificity or with no specificity. This is strikingly portrayed by Maier and Schneirla (**21**) in their highly original treatment of the phyletic evolution of differentiation, specialization, and integration in behavior.

## Summary and Conclusions

From the present state of our knowledge regarding the behavior of the newborn infant, as expressed in tabular organizations of the evidence, we may draw the following tentative conclusions:

1. From the stimulus-receptor aspect there appears to be little specificity of behavior.

2. Most of the responses tend to involve most of the major segments of the body, and the participation of smaller parts is likewise quite conspicuous.

3. Therefore, the neonate manifests highly generalized rather than specific types of behavior.

4. A quantitative expression of stimulus-receptor-effector relations seems to offer the best means of comparing one response with another at a given age level, or the behavior observed at one age period with that at another age period.

5. The extreme 'organismic' view of behavior tends to ignore the differentiation among responses which actually exists. To say in effect that in each stimulus situation it is the organism which responds is not particularly illuminating.

6. On the other hand, the view of behavior as reflex in nature tends to focus attention upon what is happening in

some particular segment or segments and to ignore what is taking place in other parts of the organism.

7. Perhaps the solution of the problem of behavioral organization will be the indication of the *relative degree* of participation of different parts of the organism. This would amount to accepting Minkowski's version of the 'reflex' as the most *manifest* aspect of the response of the organism.

## REFERENCES


1. Angulo y Gonzalez, A. W., Development of somatic activity in the albino rat fetuses, *Proc. Soc. Exp. Biol. and Med.*, 1933, 31, 111–112.
2. ——, Further studies upon the development of somatic activity in albino rat fetuses, *Proc. Soc. Exp. Biol. and Med.*, 1935, 32, 621–622.
3. Bersot, H., Développement réactionnel et réflexe plantaire du bébé né avant terme à celui de deux ans, *Schweiz. Arch. f. Neur. u. Psychiat.*, 1920, 7, 212–239; 1921, 8, 47–74.
4. Bridgman, C. S. and Carmichael, L., An experimental study of the onset of behavior in the fetal guinea-pig, *J. Genet. Psychol.*, 1935, 47, 247–267.
5. Canestrini, S., Über das Sinnesleben des Neugeborenen, *Monograph. a. d. Gesamtgeb. d. Neur. u. Psychiat.*, No. 5, Berlin: Springer, 1913, pp. 104.
6. Carmichael, L., An experimental study in the prenatal guinea-pig of the origin and development of reflexes and patterns of behavior in relation to the stimulation of specific receptor areas during the period of active fetal life, *Genet. Psychol. Monog.*, 1934, 16, 337–491.
7. ——, A re-evaluation of the concepts of maturation and learning as applied to the early development of behavior, Psychol. Rev., 1936, 43, 450–470.
8. Coghill, G. E., Individuation versus integration in the development of behavior, *J. Gen. Psychol.*, 1930, 3, 431–435.
9. ——, The structural basis of the integration of behavior, *Proc. Nat. Acad. Sci.*, 1930, 16, 637–643.
10. —— and Angulo y Gonzalez, A. W., The development of the sensory system in relation to the local sign, *Proc. Asso. for Res. in Nerv. & Ment. Dis.*, 1934, 15, 265–273.
11. Delman, L., The order of participation of limbs in response to tactual stimulation of the newborn infant, *Child Develop.*, 1935, 6, 98–109.
12. Dennis, W., A description and classification of the responses of the newborn infant, *Psychol. Bull.*, 1934, 31, 5–22.
13. ——, A psychological interpretation of the persistence of the so-called Moro reflex, *Amer. J. Dis. Child.*, 1935, 50, 888–893.
14. Furfey, P. H., Bonham, M. A. and Sargent, M. K., The mental organization of the newborn, *Child Develop.*, 1930, 1, 48–51.
15. Hunt, W. A., Studies of the startle pattern. II. Bodily pattern, *J. Psychol.*, 1936, 2, 207–213 (*Psychol. Abstracts* 4407).
16. —— and Landis, C., The overt behavior pattern in startle, *J. Exper. Psychol.*, 1936, 19, 309–315.

17. IRWIN, O. C., The amount and nature of activities of newborn infants under constant external stimulating conditions during the first ten days of life, *Genet. Psychol. Monog.*, 1930, **8**, 1–92.
18. ——, The distribution of the amount of motility in young infants between two nursing periods, *J. Comp. Psychol.*, 1932, **14**, 429–445.
19. ——, WEISS, L. A. AND STUBBS, E. M., Studies in infant behavior I., *Univ. of Iowa Studies*, 1934, **9**, 1–175.
20. JENSEN, K., Differential reactions to taste and temperature stimuli in newborn infants, *Genet. Psychol. Monog.*, 1932, **12**, 361–479.
21. MAIER, N. R. F. AND SCHNEIRLA, T. C., Principles of animal psychology, New York: McGraw-Hill Book Co., 1935, pp. xiii + 529.
22. MINKOWSKI, M., Neurobiologische Studien am menschlichen Foetus, *Hdbh. d. biol. Arbeitsmeth.*, 1928, Abt. V, T. 5B, H. 5, 511–618.
23. ——, Sur le développement, la localisation et la clinique des réflexes, *Bull. de la Société Royale de Médecine d'Égypte*, 1934, **10**, 1–20.
24. PEIPER, A., Die Hirntätigkeit des Säuglings, Berlin: Springer, 1928, pp. 102.
25. ——, Die Entwicklung des Mienenspiels, *Monatssch. f. Kinderhk.*, 1935, **63**, 39–91.
26. ——, Reflektorische Mitbewegungen bei dem Mienenspiel, *Monatssch. f. Kinderhk.*, 1936, **65**, 294–295.
27. ——, Der Saugvorgang, *Ergeb. d. inner. Med. u. Kinderhk.*, 1936, **50**, 527–567.
28. PRATT, K. C., The neonate, in 'A Handbook of Child Psychology,' (2nd ed., rev., ed. by C. Murchison), Worcester, Mass: Clark University Press; London: Oxford University Press, 1933, pp. 163–208.
29. ——, The effects of repeated auditory stimulation upon the general activity of newborn infants, *J. Genet. Psychol.*, 1934, **44**, 96–116.
30. ——, Specificity and generalization of behavior in new-born infants: a critique, PSYCHOL. REV., 1934, **41**, 265–284.
31. ——, Generalization and specificity of the plantar response in newborn infants. The reflexogenous zone: I. Differential sensitivity and effector-segment participation according to the area of stimulation, *J. Genet. Psychol.*, 1934, **44**, 265–300.
32. ——, II. Segmental patterning of responses, *J. Genet. Psychol.*, 1934, **45**, 22–38.
33. ——, III. The effects of the physiological state upon sensitivity, segmental participation and segmental patterning, *J. Genet. Psychol.*, 1934, **45**, 371–389.
34. ——, The organization of behavior in the newborn infant (abstract), *Psychol. Bull.*, 1935, **32**, 692–693.
35. ——, Problems in the classification of neonate activities, *Quart. Rev. Biol.*, 1936, **11**, 70–80.
36. ——, NELSON, A. K. AND SUN, K. H., The behavior of the newborn infant, *Ohio State Univ. Stud.: Contrib. Psychol.*, 1930, No. 10, pp. ix + 237.
37. RICHARDS, T. W., The relationship between bodily and gastric activity of newborn infants. I. Correlation and influence of time since feeding, *Human Biology*, 1936, **8**, 368–380.
38. ——, II. Simultaneous variations in the bodily and gastric activity of newborn infants under long-continued light stimulation, *Human Biology*, 1936, **8**, 381–386.
39. ——, The importance of hunger in the bodily activity of the neonate, *Psychol. Bull.*, 1936, **33**, 817–835.
40. —— AND IRWIN, O. C., Experimental methods used in studies on infant reactions since 1900, *Psychol. Bull.*, 1934, **31**, 23–46.

41. —— AND ——, Plantar responses of infants and young children; an examination of the literature and reports of new experiments, *Univ. of Iowa Studies*, 1934, 11, 1–146.

42. STIRNIMANN, F., Versuche über Geschmack und Geruch am ersten Lebenstag, *Jahrb. f. Kinderhk.*, 1936, 146, 211–227.

43. ——, Le gout et l'odorat du nouveau-né. Une contribution à la connaissance des réactions du nouveau-né, *Rev. fr. de Pédiatrie*, 1936, 12, 453–485.

44. WEISS, A. P., A theoretical basis of human behavior, Columbus: R. G. Adams & Co., 1925, pp. xv + 428.

45. WHEELER, R. H. AND PERKINS, F. T., Principles of mental development, New York: Thomas Y. Crowell Co., 1932, pp. xxvi + 529.

[MS. received March 10, 1937]

# THE PLACE OF MEANING IN PSYCHOLOGY

GLENN D. HIGGINSON
*University of Illinois*

A critical examination of descriptive and explanatory categories in current usage in some quarters of psychology reveals a rather striking anomaly with respect to the particular treatment and significance which meaning is accorded. Although one might profitably undertake the historical task of understanding this interesting matter, our present concern touches another phase of the problem. Accordingly we shall attempt to indicate briefly the particular manner in which some psychologists deal with the problem of meaning. We offer this evaluation as a direct attack upon a position which is held by various psychologists with respect to the matter of meaning and which we believe to be scientifically undesirable. We shall try to show that the treatment which meaning receives in these particular instances is undesirable (1) because it is logically inconsistent as regards fundamental principles concerned in description and explanation, and (2) because it is inadequate with respect to methodological procedure.

We turn first to Titchener because he unquestionably played a major rôle in the furtherance of a mode of thinking concerning the place of meaning in psychology which persists widely today despite the rapid development of techniques which universally stress the importance of meaning. We suggest that a keener realization by some contemporary psychologists of certain specific inconsistencies which are seemingly inherent in Titchener's treatment of the matter of meaning together with a fuller understanding of the scientific significance clearly implied in them may possibly lead these psychologists to a fuller and more complete recognition of the present-day need for a systematic approach that will so guide psychology as to avoid descriptive and methodological

inconsistencies without sacrificing either psychological fact or logical treatment.

Consonant with his emphasis upon the importance of the analytical approach to a scientific psychology and upon the necessity of describing universal and fixed structures of the mind, Titchener could find no *legitimate* place for meaning within the field of psychology. He accordingly states that "science aims at truth; it deals with facts, with the nature of things given, not with meanings." [1] As a result, he was forced to hold that the 'truth' and the 'facts' of psychology must concern solely the irreducible and static processes of the human mind, *namely*, sensations, images, and feelings. These data, he suggests, constitute the very substance of psychology. They owe their descriptive existence *solely* to the employment of a particular mode of attack. They represent the immediate resultants of the only experimental technique proper to a scientific psychology. They cannot be evaluated, scientifically speaking, apart from the specific method by which they are isolated and revealed. After dealing in elaborate descriptive detail with such structural elements for about ninety pages, he finally concludes that "sensations and simple images are all meaningless." [2]

We aim no corrective attack directly at Titchener's writings; they are finished. On the contrary, we are concerned with a persisting tendency on the part of various scientists to deal traditionally with certain descriptive categories without apparently giving thoughtful consideration either to their scientific usefulness or to the validity of the *particular method of experimental attack absolutely essential to their existence*. We hold method to be the very essence of science. If a particular method is found to be inadequate, no serious claim can be made for those resultants or classes of products directly dependent upon it. This condition, we are led to believe, applies clearly to the alleged products of analysis, *namely*, sensations, images, and feelings.

[1] E. Titchener, A beginner's psychology, New York: Macmillan, 1915, p. 4.
[2] *Ibid.*, p. 90.

Viewed in the light of his repeated insistence that meaning cannot possibly command a legitimate place in science, it is, indeed, an interesting and provocative fact that, with the exception of two other topics, *namely*, attention and image, Titchener actually lists the *largest number of index references* under the head of meaning.[3] In a statement, which might well express the position of various psychologists today, Titchener takes cognizance of the conditions which account for this particular emphasis upon meaning. "We have seen," he says, "that the whole notion of meaning is really foreign to science. When we ask, then, what meaning is, from the psychological point of view, are we not asking an irrelevant and unscientific question?" [4] He then proceeds to point out that "meaning is of great practical importance; we communicate meanings; we apprehend meanings; and we act upon meanings." [5] The question, then, he continues, "if we rephrase it a little, merely asks *that a term, familiar to us in our daily life, be translated into the language of science;* and if the translation out of common sense into science is to be made at all, psychology is the science in which the equivalent of meaning will be found. For these reasons we are justified in discussing the matter here; and the question at issue is this: What mental processes, in perception and idea, are the scientific equivalent of what we know in everyday life as meaning? *What processes carry the meaning?*" [6]

The nature of Titchener's descriptive, explanatory and methodological tasks accordingly stands clearly revealed. Such tasks must necessarily deal with something which is not scientific in order to extract something therefrom which is of a scientific nature. In short, psychology must actually proceed from the unscientific level of that which is both observable and meaningful in order to reach the scientific level where the datum is unobservable as well as meaningless. Meaningful experiences *per se* are never matters of psychology.

[3] *Ibid.*, p. 357.
[4] *Ibid.*, p. 117.
[5] *Ibid.*, p. 118.
[6] *Ibid.*, p. 118.

But meanings must be considered *at every step* simply that they may be properly reduced to their meaningless equivalents. Static parts accordingly take the place of dynamic wholes.

From sheer necessity, therefore, Titchener was forced, just as is the psychologist today who accepts his logic and mode of attack, to grapple at first hand with experiences such as perception and idea which always 'mean something.' A perception, we are told, "is always the perception of a tree or what not; an idea is always the idea of something." [7] In as much as each is unquestionably meaningful, neither perception nor idea can rightfully claim a place in psychology. Since each, however, represents a very common form of human experience, it is necessary to indicate the manner of its reduction to a scientific form and the nature of the scientific resultants. Here, accordingly, are the first steps in the logic and the methodology of this treatment. Titchener's next steps bring him to the inevitable conclusion that "all perceptions and ideas may be analyzed." [8] They place him at the same time definitely across the scientific Rubicon from which retreat becomes impossible save at the immediate loss of a vast amount of scientific territory. It is quite clear that any failure *now* to analyze meaningful experiences must show either (1) that his fundamental position and his basic mode of attack are scientifically untenable, or (2) that any further dealings with that large province occupied by perception and idea, *which he unquestionably admits are always meaningful*, must be completely abandoned. In as much as psychology, as science, cannot treat directly with meanings by way of description and explanation, but must confine itself to *meaningless sensations and images*, there can be no psychology where the latter are lacking.

In this particular respect, Titchener was at least perfectly straightforward. His position is to be contrasted, however, with that of certain contemporary psychologists who, in the earlier chapters of their basic expositions of psychology, deal

[7] *Ibid.*, p. 113.
[8] *Ibid.*, p. 114.

in considerable detail with matter of sensations and images as if they were basic phenomena and who then turn in following chapters to discuss some topic, supposedly significant for an understanding of life situations, such as motivation, reasoning, intelligence, learning or personality without, however, referring *in any manner whatsoever* to the previously discussed materials. In all these cases, the chasm between the earlier and later, between that which precedes and that which follows, is seemingly so wide that neither such psychologists nor their students ever successfully bridge it. We can, at least, say that Titchener has furnished us both an example and a warning.

After crossing the Rubicon, Titchener discovered the 'enemy' to be extremely elusive as well as stubborn. It is indeed significant that, although he repeatedly investigated meaning in its most important aspects, he obtained neither sensations nor images. On the contrary, he found that he actually had at the end what he had at the beginning. There was only meaning. We find him stating, therefore, when all frontal attacks by way of phenomenal analysis had completely failed, that "meaning is carried not by any sort or sensation or image, but simply and solely by physiological processes, by some set or disposition of the brain." He then goes on to remark that "when the practised reader skims a number of pages in quick succession; when the musician renders a composition in the prescribed key; when an accomplished linguist shifts from one language to another as he turns to his right or left hand neighbor at a dinner table; in cases of this kind there need be no discoverable context; *the stimuli press the button, and the brain, prepared by constant practise in the past, now does the rest* . . . the nervous system can now do, by a set or disposition that has no mental correlate, what it used to do by processes that had as accompaniment the sensations or images of the context." [9] (Italics ours.) Of this significant fact, then, following Titchener, we may be quite positive: The brain itself may act directly 'without mental intermediates of any sort' to produce a

[9] *Ibid.*, p. 120–121.

(meaningful) perception or idea. While there may possibly be 'sensations and images' accompanying some meaningful experience, it should be fully understood that Titchener, himself, was never able to reduce a meaningful phenomenon so that at the end of analysis there remained nothing but meaningless sensations and (or) images.

In all these cases, where mental analysis failed, where description of meaningless elements was wholly impossible and where scientific explanation by necessity rested directly upon such antecedent agencies as 'habitual nervous set,' 'directive nerve-force,' 'brain disposition,' Titchener did not directly proceed to discard perceptual or ideational meanings as being unscientific and improper materials for psychological treatment. As a matter of fact, in his treatment of the *perception of distance*, he not only rejected an analysis into 'depth sensations,' but specifically criticized those psychologists who seek to 'call in the kinæsthetic sensations from the muscles of the eye.' According to their thinking, he says, "*depth or distance is psychologically a blend or fusion of visual and kinæsthetic sensations.*" [10] But of such an 'analysis' of this meaningful phenomenon, Titchener critically remarked that "No one can say positively that this hypothesis is wrong; but it is difficult to believe that the blend of visual and kinæsthetic sensations should yield a result so different from either —namely, the perception of space. It seems safer to say that *the binocular picture*, the appearance of a book to the two eyes or the combined image of the stereoscope, *carries the immediate meaning of depth or voluminousness.* The picture is not itself deep or solid, but we cannot help perceiving it *as* deep and solid; and this pressure is laid upon us by what we have called racial heritage, an inherited disposition of the nervous system; *the brain meets the impression halfway.* The binocular picture thus becomes the core or nucleus of the perception; and the meaning of depth is carried by a nervous disposition that has no correlate in sensation or

[10] *Ibid.*, p. 129.

image." [11] "The meaning of depth is carried, in the last resort, by brain-habit." [12]

These cases indicated by Titchener in which the *nervous system functions directly under stimulation to produce observable meaningful phenomena* are not to be understood as constituting rare exceptions. When he discussed other forms of perception, Titchener again continued, in absence of all discoverable analytical mental products (sensations, *etc.*), to draw directly upon *brain function under environmental stimulation* as the sole basis of his observable and describable scientific materials. In a discussion of the meaningful experience of visual movement, he remarks in part that one can "see movement, where there is no movement to see! Here, then, is a case of perception of movement in terms of *sheer brain habit, of a settled nervous disposition that now has no mental correlate*, but whose establishment has depended on the past history of the individual, possibly of the race." [13] A similar position is indicated for the variety of perceptual experiences concerned with melodies and illusions. Of the latter, he queried specifically concerning the adequacy of 'analysis' and proceeded to answer his own question by saying that illusion 'involves a brain-habit' and 'is found to vary with our general attitude toward the figures.' [14]

For final illustrations of the significant lack of all analytical products, where such would logically seem to be vitally necessary for scientific description, we turn to the chapter on memory and imagination. Here again we find Titchener saying that meaning, "a contextual setting of some bit of bare experience, may in course of time be carried by nerve-processes which have no mental correlates of any kind. The same thing seems to hold of recognition." [15] He then continues to say that "in experiments on the recognition of greys, the author has reported positively that a particular grey had been seen before, without being able to find anything

[11] *Ibid.*, p. 129–30.
[12] *Ibid.*, p. 131.
[13] *Ibid.*, p. 133–34.
[14] *Ibid.*, p. 137.
[15] *Ibid.*, p. 181.

whatsoever, in the way of verbal idea or kinæsthetic quiver or organic thrill, that might carry the meaning of familiarity; the brain-habit just touched off the report 'Yes,' and that was all that could be said." [16] An examination of remembrance and recollection reveals that there is 'something,' 'an intent' in the background "that holds us to a particular circle of ideas" (meanings). In the absence of discoverable products of analysis, Titchener finally suggested that "this something may be a group of contextual mental processes or a nervous disposition." [17]

Our particular examination of Titchener has necessarily been brief, but we believe it to be fair and representative. It holds directly for Titchener's position and indirectly for all those psychologists who hold either by statement or by implication that meaningful phenomena can be reduced to meaningless parts. It is, in fact, entirely representative of those particular psychologists who maintain, at the expense of logical consistency and methodological adequacy, that there are a few classes of irreducible meaningless mental elements, *namely*, sensations, images and feelings which constitute the basic materials of psychology and to which psychological phenomena can be reduced by use of the method of structural analysis. Despite the obvious lack of any need other than that of adhering to tradition, current psychology goes blindly ahead in some quarters trying vainly to ride two ill-mated steeds, *namely*, meaningless element and meaningful experience and actually succeeding only in riding the one until its inadequacy is so grossly apparent that, if further expository progress is to be made, a switch to the other becomes imperative. No psychologist—either Titchener or any one of those who have reflected his influence from this particular angle—has been sufficiently adept, however, to make the switch without losing every bit of his worthwhile scientific luggage.

The lesson which Titchener taught psychology is so simple and so striking that any one who regards tradition lightly

[16] *Ibid.*, p. 182.
[17] *Ibid.*, p. 190.

might rightfully express surprise that it has not been universally learned. Titchener indirectly indicated a way of avoiding the inconsistency that necessarily adheres to this dichotomy of meaningless and of meaningful materials. That he seemingly discovered this particular way as a result of sheer necessity and not by choice can in no way imperil the full scientific security which it offers psychology. Titchener repeatedly pointed out that meaning must be regarded as arising as a *direct resultant of a human brain functioning under extraneural stimulation.* Despite his basic assumption that all meanings (perception, ideas, *etc.*) are reducible to meaningless correlates, he was repeatedly forced to substitute in their stead sheer nervous functions entirely bare of all mental processes. If Titchener had originally *started* with the basic postulate that the brain functions without any intermediate meaningless process such as sensations and images to serve as agencies to *produce* meaningful observable phenomena, we may safely assume that he would then have avoided the particular way which he actually took with its inescapable pitfalls of methodological inadequacy and systematic inconsistency. And, at the same time, psychology would have lost nothing and gained much. The same assumption must necessarily hold for those psychologists of today who continue to refer vaguely to sensations and images and feelings as simple mental correlates or meaningless components of meaningful experiences as if in the hope that the reader or the student will possibly find some connecting relationship.

Two clean cut examples of Titchener's influence upon psychological thinking of today may be found in two recently published elementary texts. In the one case,[18] we find the approach previously emphasized by Titchener in which mental elements are treated as if they constituted the basic raw materials out of which the whole fabric of mental life is actually woven. Yet an examination of the descriptive and explanatory treatment of such fundamental topics (chapters) as spatial relations, temporal perception, perception of movement, nature of perceiving, learning, emotion, action and

[18] E. Boring, H. Langfeld, H. Weld, Psychology, New York: Wiley and Sons, 1935.

personality significantly reveals no use whatsoever being made of such postulated mental elements. Whole chapters in this presentation accordingly fail to show any significant reference to sensations or images as basic processes. On the contrary, we find statements such as the following which clearly run at an entirely meaningful level: "The familiar objects about us belong to a spatial world and we perceive them as having length, breadth, and, in many cases, depth. Furthermore, these objects are localized." [19] Here, then, are observable, describable and measurable properties of environmental objects—properties which are unquestionably psychological. Again, we are told that the perceptions of size, distance, direction of objects "are immediate; generally they are compelling in the sense that we cannot see them in any other way, and it does not in the least change the perception to know its conditions." [20] Such statements accordingly imply that man's environmental objects possess definite, definable and meaningful properties which cannot be reduced to nor explained in terms of the categories of more basic (simpler) phenomena.

In the second case,[21] we find in the earlier chapters that the author deals in considerable detail with the nature of mental elements or processes. Following thirteen chapters of such consideration and with nine chapters still remaining in the exposition, a complete shift in expository treatment appears. "One last word," thus Fernberger writes above about the middle of his book, "before we leave the discussion of the problems of consciousness. We have found that no conscious process ever just happens spontaneously. Each requires some immediate external physical stimulation acting upon the nervous system to set them going. Some external stimulus must supply the energy which eventually stimulates some portion of the brain surface so that this stimulation will result in consciousness as sensation, image, or a train of

[19] *Ibid.*, p. 207.

[20] *Ibid.*, p. 235.

[21] S. Fernberger, Elementary general psychology; Baltimore: Williams and Wilkins, 1936.

images as in thinking."[22] Fernberger then continues throughout the remainder of the exposition with a discussion of various problems of emotion (fear, phobias), volition (suggestibility, hypnosis, hysteria), habits, reactions, reflexes, instincts, motivation, individual differences and personality. But in this treatment of some of the most fundamental problems of psychology, there appears no consideration of such mental processes as simple sensations and images.

It should be noted that despite his lengthy discussion of matters which are universally recognized as being meaningful, Fernberger has little taste for meaning. The reasons which he cites are indeed interesting, especially in view of the highly questionable states of sensation and image as simple bits or parts of mind. "Until now we have not spoken of the psychological problem of *meaning*, partly because the psychology of meaning is still in a very unsettled state and the psychological explanations of meaning are none of them complete or satisfactory. *We have spoken throughout of stimulus objects as if they were meaningful objects.* (Italics ours.) The reaction of meaning and of mental process is by no means as clear cut as this however."[23] He then goes on to say that "a relatively indefinite psychological process, with the adequate apperceptive background, may be very highly meaningful."[24] In another place, we are again told that "important as the problem of meaning has proved to be, one must confess that there exists today no adequate explanation of meaning nor an adequate psychological explanation of the relation of the conscious processes of sensation and image to meaning."[25] On the same ground, one could just as readily and as logically throw out all sensations and images.

Evidence of a growing tendency among other students who have followed Titchener, to emphasize the significance of meaning and the rôle of brain functions, is found in Pillsbury's 'Fundamentals.'[26] While discussing attributes of sensation,

[22] *Ibid.*, p. 257.
[23] *Ibid.*, p. 88.
[24] *Ibid.*, p. 88.
[25] *Ibid.*, p. 253.
[26] W. Pillsbury, Fundamentals of psychology, New York: Macmillan, 1934.

such as extent and duration, Pillsbury drops an early hint that: "In the simplest cases, these ways of varying seem immediate and unanalyzable, but in many more instances, it is possible to show that the appreciation both of extent and of duration depends upon more complicated mental operations." He then makes the significant statement that "*They seem to belong to the object rather than to the sensations as such.* In consequence it is more convenient to treat them both in connection with perception, as a way of interpreting objects, than to consider them as attributes of sensation." [27] A short time later, he comes specifically to the point of issue, *namely*, the nature of meaning and of mental process (content). The former, we find, is a matter of straightforward observation; but the latter is actually a matter of theoretical conjecture. "We are aware of the meanings first," he says, "and then *build up theories of the presence* of sensations to explain how the meaningful experience originates." We are aided in securing an understanding of what Pillsbury believes to be the character of these assumed 'sensations' and 'images' by the illustration which he gives concerning the observed size of an environmental object. "So far as the adult can remember he has always seen the object as of the same size no matter at what distance it may have been. He knows that the perception *depends upon images on the retina* only after he learns a good deal about the *retina and the physics of light.* . . . Similarly a cross always seems to have a right angle no matter from what direction it may be seen or how it may turn. A bit of paper always seems to be of about the same shade whether in sunlight or shadow, although its *actual brightness varies strikingly.*" [28] Finally in discussing the unit of experience he remarks that "The very fact that one *knows so little of the character of one's images* and is so *sure of what they represent is sufficient evidence that what is* real is the idea intended rather than the images." [29]

[27] *Ibid.*, p. 101. (Italics ours.)

[28] *Ibid.*, p. 390–391 (Pillsbury is guilty of a very common slip here. As we understand him, he is guilty of using 'actual' as synonymous with 'physical.' The matter is very different, if he uses 'actual' as synonymous with 'psychological.' However, Pillsbury is not without company in this strange usage).

[29] *Ibid.*, p. 398. (Italics ours.)

A most striking illustration of the inadequacy of any theoretical approach which emphasizes mental content appears in connection with thinking. Careful experimentation by skilled observers has repeatedly failed to reveal the slightest trace of meaningless elements that might serve as the 'mental correlates of meaning.' In this particular connection we point to the work of Külpe, Ach and others. Their experimental results indicate that meaningful (relational) properties directly emerge and can be described in the complete absence of any observable mental contents. Meaningful problems are accordingly followed directly by meaningful solutions. In view of all such studies, one may well be surprised to read in a modern discussion of thinking that "in normal thinking one is concerned with the meaning and not usually with the particular images in consciousness. It is only when one becomes interested in psychology that he is concerned as to whether a given meaning is present in consciousness in terms of a visual concrete image or a kinæsthetic image." [30]

In closing, we wish to emphasize the point that psychology today faces two possible courses either of which it may follow. It may possibly continue in its attempt to struggle along unnecessarily burdened with its half-meaningless and half-meaningful material although no determinable relationship between the "halves" has been shown, or it may discard the meaningless half and move unhampered along a wholly meaningful course. This way is known to everyone familiar with 'applied' psychology. Along this course, it will face one major set of problems, that of the description and explanation of meaning. As it proceeds, it will always deal directly with wholly observable phenomena and their causes. These phenomena, properly regarded, are the psychological properties of objects and situations. Such properties or characteristics of things are the products of a functioning brain. In common with other products, they can be localized and described. They are simply the psychological resultants of a dynamic organism. They emerge as the effects of a brain which is

[30] S. Fernberger, Elementary general psychology, Baltimore: Williams and Wilkins, 1936, p. 249.

activated by environmental energy (stimulus) or *of a brain that functions independently of stimulation* because it is the very nature of man's organic structures (organs) to function always and continuously at some level (or levels) of activity. We hold by necessity to the assumption that *to function* is an inherent characteristic of man's brain. There may or may not be extraneural energy acting upon it; but it always functions. The sole products of such functioning, as far as we are concerned, are meaningful (psychological) properties of world objects.

If one holds to meaningless processes, the difficulty of his task is materially increased. He must accordingly deal with things which are unobservable, non-localized, and non-meaningful. He must describe them purely in theoretical terms, he must show how they are related to meaning, and he must explain how such processes actually exist. Furthermore, he is faced with the task which so troubled Titchener, namely, of dealing with all those fundamental experiences in which no such simple mental processes can be discovered—of which no analyses have ever been made. He must necessarily show why so many of man's experiences, as Titchener and others have indicated, can be successfully approached only from the meaningful level, yet at the same time make clear why other experiences can (allegedly) be approached at a meaningless level. In short, he must face the unavoidable task of reconciling a purely theoretical position with solid experimental findings. If in so doing, he places theory above fact, he can scarcely hope to command a thoughtful scientific audience. Logic and scientific progress today demand of thoughtful psychologists much more than traditional adherence to a theoretical approach that entails both descriptive inconsistency and methodological inadequacy. They demand a way that lends integration and significance to those life phenomena of a psychological nature. Such a way can run only at a meaningful level.

[MS. received May 3, 1937]

# REVERSAL OF AUDITORY LOCALIZATION

BY PAUL THOMAS YOUNG

*University of Illinois*

The study of Willey, Inglis and Pearce (5) upon reversal of auditory localization through the use of a pseudophone confirms in most respects the earlier work of the writer (7). There are, however, one or two discrepancies in fact and interpretation. The present paper is a comparison of these two studies for the purpose of pointing out the basic agreement between them and to reinterpret the differences.

We will begin by presenting the main facts of reversed auditory space. Discussion of these facts will be given later.

## The Facts

1. *Right-left reversal of auditory localizations.*—The pseudophone produces a persistent right-left reversal of auditory localizations. In presenting their results Willey, Inglis and Pearce write: "Purely auditory localization, or localization without visual knowledge of the position of the sound-producing object, was tested at the beginning and toward the end of the experiment. No change in localization of this type could be detected" (5, 122). This agrees completely with our previous result that, "From first to last purely auditory localizations were reversed" (7, 413).

It is clear that when the stimulus was entirely an acoustic one the pseudophone yielded consistent reversal of auditory localization. In the laboratory when the subject's eyes were closed reversed localizations were found at all stages of habituation.

2. *Kinds of reversal.*—If we imagine the median plane of an observer to be a two-faced plane mirror, then, in a common form of reversal, the phantom has approximately the same relation to its source as the mirror-image has to its real body. This mirror-image relation has been called 'simple lateral

reversal.' But there is another kind of reversal best described by stating that the phantom is approximately 180° removed from the source, assuming the center of the head to be the center of auditory space. In this second kind of reversal there is a front-back as well as a right-left inversion.

Willey, Inglis and Pearce report that, "For all positions front-back and back-front reversals were at least as generally observed as simple lateral reversal" (5, 121). The writer also noted that the 180° type was more frequently experienced (7, 420–2). Precise quantitative comparisons, however, were not made in either study.

There are probably other kinds of reversal than these two. Willey, Inglis and Pearce make this interesting report: "The sound of an airplane in the air generally was not experienced as a complete 180 degree reversal, although lateral shifts of localization were regularly observed. One subject, however, reported an instance of diametrical reversal of the sound of an airplane; on this occasion the characteristic sound seemed to be coming out of the ground" (5, 120).

The two studies agree that right-left and left-right reversals invariably occur when the pseudophone is worn and when the only basis for localization is the acoustic stimulus. These lateral reversals are frequently combined with simultaneous front-back or back-front reversals. Up-down or down-up reversals may or may not occur.

In a subsequent investigation (8) the writer demonstrated that discriminations between front and back, and between up and down, cannot be made when head movements are ruled out as factors in auditory localization. If head movements are rendered ineffective, *i.e.*, if they can in no way change the binaural stimulus-pattern, the phantom takes its place upon an arc which lies above the level of the ears and outside of the visual field. Auditory space becomes bidimensional.

3. *Localizations thrown backward.*—Willey, Inglis and Pearce have reported that: "Under the conditions of normal hearing all three subjects showed a tendency to give a preponderance of 'front' judgments for an equal number of front and back presentations of the stimuli. With the pseudophones the opposite tendency was noted" (5, 119–20).

The present writer described this phenomenon as follows: "The median plane presents a special case. When the source is due front it is generally localized as due back. When the source is due back it may be localized in front but most frequently it is localized in back. In other words, localizations in the median plane tend to be thrown towards the rear. This suggests the normal tendency to throw localizations forward" (7, 422).

4. *Reversal of motor adjustments and localizing responses.*—The overt localizing responses were at times reversed. Willey, Inglis and Pearce write: "Numerous incorrect motor adjustments were made, particularly at the dinner table, where the complexity of the sound stimulation was especially favorable to confused localization. . . . After the pseudophones had been worn for a time, a conscious correction of movements became the rule when it was necessary to utilize auditory cues exclusively. Whenever the observer was startled the immediate motor response was always to the wrong side" (5, 121).

This confirms our findings completely (7, 409).

5. *Normal localization of sound with the eyes open, despite the pseudophone.*—Willey, Inglis and Pearce write: "When the observer can see the source of sound, it was found in the experiments of Stratton, Young, and Ewert, and in the present experiment, that the sound sometimes seemed to be coming from the position of the seen object" (5, 123).

On the surface this statement appears to agree completely with our finding, quoted below: "One of the most interesting features of the present experiment is the occurrence of normal localizations despite the transposition of the ears. The preliminary experiments showed that, at the very start, fixation of the source, or attention to it, sometimes brought about normal localization. This occurred repeatedly at all stages of the present experiment. Thousands of cases were not recorded" (7, 410).

Willey, Inglis and Pearce, however, explain that the normal localizations which occurred were of a 'special quality' which might better be described as 'a temporary repression of

the auditory cues.' They continue: "Visual knowledge of the position of an object, sometimes supplemented by long habituation to the coincidence in space of an object and its characteristic sound, may sufficiently over-ride the auditory cues so that the observer is not aware of any discrepancy between the visual localization of the object and the auditory localization of the sound. To conclude from this circumstance that auditory localization with vision has become normal seems definitely without warrant" (**5**, 124–5). They resort, moreover, to other explanations than 'repression'; to attention, association, empathy.

In view of their discussion of normal localization it seemed worth while to go into the matter in some detail. We therefore reexamined our original notes to read at first hand the evidence for normal localization with the pseudophone. We were impressed with the fact that all nine of our control subjects reported normal localizations when their eyes were open. Some notes upon their reports are reproduced below:

Observer K. Lewin: With eyes closed right and left were regularly and uniformly reversed regardless of the direction and distance of the source from the head. With eyes open localizations were normal when the source was in front and at the sides. Once while Lewin was listening to some sounds made by rapping on wood with a xylophone hammer the experimenter spoke; his voice was inverted in localization but the other sound was normal throughout. When the source was near to the ear localization was inverted; at a greater distance the localizations were sometimes normal, sometimes uncertain.

Observer J. F. Brown: With eyes closed right and left were consistently reversed. With eyes open the sound was usually heard where the object was seen. Brown was surprised at the normal localizations and commented specially upon them. While listening to the sounds made by rapping on wood the experimenter spoke. The rapping sounds were reported as normal but the speech was reversed. Brown said, 'To a certain extent I can put the sound where I will.' With a watch held near to the trumpet the localization was reversed despite disagreement with the position of the visible watch.

Observer C. R. Griffith: The subject was tested with eyes closed; all localizations were right-left transposed. With eyes open

there were sometimes two localizations of the rapping sounds—one where the object was seen and the other 180° from there. At one time the rapping was normally localized but the voice of the experimenter was reversed.

Observer Frl. T. Dembo: With eyes closed all localizations were reversed. With eyes open the noise made by a door suddenly and unexpectedly opened at one side of the room was clearly reversed. With eyes still opened the sounds made by rapping on wood were normally localized in various positions; at the same time when the experimenter spoke his voice was reversed in localization. There were both normal and reversed localizations of sound from sound-waves originating at approximately the same position in physical space. Sometimes the voice was heard at the place of the speaker as it normally is; at other times the speaker was seen at one place and the voice heard 180° from there. In listening to and looking at a wall-clock the ticking sounds were heard in their normal position, but when the experimenter suddenly spoke from about the same position his voice was clearly and consistently reversed.

The above accounts with minor verbal changes are taken directly from the original notes made during the observers' first experience with the pseudophone. Nine subjects gave consistent reversal of localizations when their eyes were closed; all reported some experiences of normal localization when their eyes were opened and the source visible.

The same result was found by Klemm (**3**) whose paper we have previously quoted (**8**, 123).

All things considered, it appears to be well-established that the subject who consistently reverses his sound localizations with eyes closed, reports both normal and reversed localizations when his eyes are open. With nine control subjects both normal and reversed localizations were reported during the first few minutes after the pseudophone had been adjusted; if we are to take their words at face value for the reversed localizations, we should also take them at face value when they report normal ones.

When normal localizations were first observed we thought that there might be sound leakage around the ear-pieces and took special precautions against it; but the fact that all

localizations were consistently reversed when the subject kept his eyes closed is conclusive evidence against the leakage hypothesis.[1]

One possible explanation of the 'special quality' of normal localization to which Willey, Inglis and Pearce refer is that these investigators did not distinguish clearly between (*a*) the indefinite, vague localization with no awareness of discrepancy, and (*b*) precise localization, both normal and reversed. Indefinite localizations are described below.

6. *Indefinite localization and meaningful identification.*—In describing the main characteristics of sound localization with the ears acoustically transposed the writer mentioned the occurrence of sounds which were indefinite in localization—just off at the right or off at the left or wholly unlocalized—but which nevertheless were identified significantly with their visible sources (7, 412). These indefinite localizations and unlocalized sounds were distinguished from the definite ones.

Spatial meaning (knowledge of the true position of the source) was repeatedly distinguished from spatial localization or hearing the sound as *here* or *there* (7, 418). Judgment of the position of the source sometimes agreed and sometimes disagreed with the localization.

Because these distinctions are of critical importance in analyzing the problems of reversed auditory localization, some additional excerpts from the original notes are here reproduced:

> Street noises—the sound of horses' hoofs against the pavement, the rumbling and bells of cars, the hum and horns of autos, etc.—are referred to their obvious and objectively correct sources, but the ear seems to play very little part in this. There is a sound (tread of horse on cobble stones) and an obvious source (moving horse). The visual and auditory phenomena are identified but there is no true auditory localization; auditory localization plays

[1] It is to the credit of Willey, Inglis and Pearce that they made a quantitative study of sound leakage and analyzed the total binaural stimulus-pattern from the standpoint of leakage. We were aware that there was some leakage of sound around the ear-pieces, and controlled it so far as possible (7, 401).

For the benefit of future investigators the writer suggests that sound leakage might be further reduced through the use of heavy felt pads, cut out to accommodate the ears and held in place by a helmet resembling the kind used by aviators to reduce noise of the motor, or by a similar sponge-rubber cushion.

little if any part in the process. Although the sounds are clearly heard and referred to their normal places, there is no sharp localization of sound, but only a general reference of the sound to its source based on knowledge of the situation.

There are many instances of indefinite localization. The sound of a wagon as the wheels roll over the cobble stones is referred to *that* general region and to the cobble-stone situation in particular. There is no clean-cut auditory localization of the sounds exactly at that point. Taking all things together, *that* is where the sounds belong, where they *are*.

There is an experience closely akin to localization of the following kind. Sounds are clearly heard and recognized as 'someone pounding,' 'clock ticking,' 'street car approaching,' etc. When the source is seen or its position known the sounds are immediately and without reflection identified with the source. If the experience could be put into words, it might be formulated this way: "It is the man on a ladder with a hammer I hear pounding," "It is the clock over there I hear ticking," "It is that street car I hear approaching," etc. These experiences give a clear impression of sound localization; sound and source are identified convincingly and there is no felt inconsistency between visual and auditory experiences. The experience was very frequent.

When I fixated the clock with eyes open there was complete indefiniteness of localization. The ticking was not definitely reversed nor was it definitely normal. The best description I can give is to say that the ticking was nowhere in particular but that it belonged to the fixated clock. The clock and the ticking were meaningfully identified and no inconsistency was felt. There was no clean-cut definite auditory localization.

The above excerpts make it clear that the *non-awareness of discrepancy* between source and sound was distinguished from *normal localization*. These indefinite spatial experiences did not lead us to conclude that auditory localization with vision was normal. They were regarded, however, as transitional between the normal and reversed forms of localization.

7. *Double localizations.*—We found that sometimes there were two simultaneous localizations—one normal and one reversed—for a single auditory stimulation (7, 411). There is no reference to double localizations in the report of Willey,

Inglis and Pearce; but the phenomena are of considerable theoretical importance.

8. *Normal localizations obtained through searching movements when eyes are closed.*—During the main experiment the writer found that reversed localizations were not changed into normal by head and body movement, but that they remained reversed throughout.

In a supplementary experiment, however, normal localizations were sometimes obtained through head movements when the eyes were closed. Such normal localizations occurred only under very special conditions. A ticking clock was attached to one end of a beam which rotated freely above the subject's head. From time to time this beam was grasped by the hand and held in a fixed position. The clock was then known to be located at one end of the beam or the other. By movements of the head, changes in loudness and timbre were observed which gave unmistakable evidence of the actual position of the source. The position of the source could, therefore, be correctly judged on the basis of auditory cues alone. Further, the subject had the definite aim of localizing normally if possible (7, 415–19).

It was found under these special conditions that sometimes there was disagreement between: (1) judgment of the position of the source, and (2) sensory localization. The source was *known* to be here but *heard* there. Sometimes, however, sensory localization was normal and agreed completely with the judgment.

Relative to this experiment Willey, Inglis and Pearce write: "Normal localization by means of head movements, such as Young reported, was obtained by none of the observers in the present experiment. On the contrary, it was found that head movements, together with movements of the whole body, produced *more exact reversals* . . ." (5, 122). After pointing out that Young reported a type of lateral reversal which was unaffected by movement, they state that ". . . one is hardly justified in concluding that head and body movements ordinarily make possible correct localization when the pseudophone is being worn" (5, 122).

This last statement is correct: under *ordinary* conditions head and body movements did not restore normal localization. But the very *special* conditions under which some normal localizations were obtained by Young through head movements were not repeated by Willey, Inglis and Pearce. Young's statement, therefore, that normal localization of sound may be obtained through head movements, stands as a positive result which is supplementary to the main conclusions, neither confirmed nor discredited by the work of Willey, Inglis and Pearce.

9. *Development of normal localization with eyes open.*—Both experiments agree that purely auditory localization was reversed from first to last; it did not develop (See section 1 above). When the sound stimulation came suddenly, unexpectedly, or from a source outside of the visual field, reversed localization was usual at all stages of the experiment. Through the control of observational attitude with eyes open reversed localization could be heard at any time during the experiment, as in the observations with a metronome (7, 402–3). Frequently when in a state of abstraction the writer noticed reversed localizations, even after much habituation.

Willey, Inglis and Pearce have stated that such normal localizations as occurred with eyes open *did not develop*. They write: "Instances of reversed localization which was unaffected by visual knowledge of the source of sound were reported at all stages of the experiment, and instances of normal localization were similarly distributed. Some of the most striking reversals occurred toward the end of the experiment" (5, 124).

In contrast with the last statement Young has reported that habituation to pseudophonic localization *developed* the visual-auditory type until indistinguishable from normal. He meant that as the experiment progressed normal localizations became more frequent, more certain, and convincingly normal. Confusion, indefinite localization, dissociation of sound from its visible source, were less frequent at the close of the experiment than at its start. This development was stated as an *impression*, no quantitative evidence being available (7, 413–15).

The view of Willey, Inglis and Pearce that visual-auditory localization did not develop is also an *impression*. What are the facts? Before we can be certain we must define our basic conceptions and plan experimental series which will yield quantitative results bearing upon the development of visual-auditory localization.

In the meantime the disagreement concerning the nature and development of normal visual-auditory localization should not obscure the great bulk of findings upon which the experiments agree.

## Discussion

As noted above (See section 5) normal sound localizations sometimes occur despite the pseudophone when the subject can see the source. To account for this fact Willey, Inglis and Pearce have referred to a number of conventional psychological principles:

(*a*) *Attention and change of observational attitude.*—When the subject found that sound localizations were not dependable he ceased depending upon them and relied increasingly upon vision. Attention to the source gave normal localization but when attention was directed to the sound the latter was invariably reversed (**5**, 124). Normal localization is thus dependent upon a direction of attention.

There may also have been a change in observational attitude rather than any fundamental reorganization of auditory perception (**5**, 125).

(*b*) *Repression.*—Visual knowledge of the source plus long habituation to the coincidence in space of an object and its sound, may over-ride the auditory cues so completely that the observer is unaware of a discrepancy between sight and sound. Through repression the subject may be unconscious of the dissociation without any alteration in the cues themselves (**5**, 124–5).

(*c*) *Association.*—A strong association between the usual tone of voice and the visual representation of the speaker may counteract the auditory localizing cues. This is especially obvious in conversations. If the speaker laughs, coughs, or

suddenly changes his tone of voice, there is at once a reversal of localization, even though the localization just previously had seemed normal (**5**, 125).

(*d*) *Empathy.*—The voice of a person with whom the subject is conversing is usually localized normally when the speaker can be clearly seen, and especially when the movements of lips and facial muscles can be followed. This was so compelling that when engaged in conversation "it was necessary for the observer to consider very carefully from what direction the sound seemed to be coming to perceive reversed localization, and even with close attention a definite reversal was not always perceived" (**5**, 128).

(*e*) *Visual suggestion* is mentioned in passing as a possible explanation of normal localization of sound under conditions of spatial disorientation (**5**, 127).

The above principles of explanation are mentioned incidentally by Willey, Inglis and Pearce; they are not considered in detail. We should note, however, that all of them stress *central* rather than *peripheral* factors in the localization of sound.

*Related experiments.*—The studies we are considering are related to the experiments of Stratton (**4**), Wooster (**6**), and Ewert (**2**) upon disorientation of vision.

Stratton with his inverting lenses found that sound localizations tended to follow the position of the visible objects. Ewert, employing a similar technique, found this to a limited extent. One of Ewert's subjects described an experience strikingly similar to that frequently met with the pseudophone: "A car was heard to come from the right, moving toward the left. As soon as it entered the field of view the sound jumped over to the opposite side. It was now both seen and heard to travel toward the right. However, as soon as it passed out of the field of view the sound seemed to jump back and continue its course toward the left" (**2**, 352; compare with our report, **7**, 408).

With other subjects, however, sound localization did not shift; vision was regarded as a *distraction* to the sound localization.

When the source was in the non-visual field continuous inverted vision had a neutral effect upon sound localization, according to the findings of Ewert (**2**, 356). Sound localization as such under these conditions had no tendency to swing into line with visual localization. When the visual distractor was removed sound localization was at once normal.

This last statement should be compared with the findings of Young and of Willey, Inglis and Pearce that pure auditory localization as such was very resistant to change. When eyes were closed sound localizations were reversed from first to last; and when the pseudophones were removed there was an instantaneous and complete return to normal localization despite long habituation to the reversed conditions.

It is only when spatial disorientation brings some maladjustment within the subject's world that the changes we are mainly considering occur.

Wooster—using a pair of prismatic lenses which produced an angular deviation in the visual field of about 21 degrees—investigated the return to normal of manual and auditory localizations. She concluded that sound is not an effective factor in the formation of new spatial coördinations (**6**, 92–3).

A survey of the experiments with visual disorientation justifies the conclusion that auditory localization is not changed when there is no experienced discrepancy between the visual and the auditory localizations. Even when there is a discrepancy the results are lacking in uniformity; sometimes the sound takes the position of the visible source and sometimes it does not.

There are thus individual differences in the extent to which visual knowledge of the position of the source determines auditory localization, as Willey, Inglis and Pearce have pointed out (**5**, 126–7). What is the nature and explanation of these differences?

It is possible that there were motivational differences. Throughout his experiment the writer was eager and active to localize normally, if possible. He made repeated attempts to "educate" the space behind his head by attending to some sounding, moving source as the object left the visual field

and by then expecting the source to appear on the opposite side. For example, a motor-cycle would be followed by eye and ear until it left the visual field behind the back; then it would be followed by ear alone and later by eye and ear as at the start. In such situations with a little practice the sound remained normal throughout. At the start it was necessary to look attentively at an object to localize the sound normally, but with habituation the merest glance or passing consideration served to check the fact that the sound was normally localized.

In the experiment of Willey, Inglis and Pearce it is not certain that the subjects had the same eager determination to hear normally and the same persistence in training that the writer had. It is understandable how a relatively passive attitude of observation might give the impression that normal localization did not develop. In laboratory situations where nothing of importance depends upon normal sound localization it is easy to understand how a discrepancy between visual and auditory space might persist indefinitely.

*Normal localization and its development: a restatement.*— Normal localization with the pseudophone depends upon a central adjustment to the position of the source rather than upon peripheral auditory cues. In a special experiment one ear was tightly plugged and the opposite ear connected to the pseudophone so that the sound-receiving trumpet for the hearing ear was located on the opposite side of the head. Under these conditions with eyes open normal localization was easy to obtain; the sound was distinctly and precisely heard where its source was seen to be. The phenomenon was so convincing that an assistant was called to check the localizations with eyes closed. It was a definite shock to find that with eyes closed the monaural localization was completely unreliable, no better than by chance. The incident is cited here merely to show that an adjustment to the true position of the source and a determination to localize sounds normally brought the most convincing normal localizations upon a monaural basis; but that when eyes were closed objective

accuracy and certainty of localization were at once removed (7, 404–5).

The experiments of Klemm, Young, Willey, Inglis and Pearce, as well as observations made by Stratton and Ewert, agree that visual fixation of the source produces normal sound localization when visual and auditory localizations have been artificially dissociated. This indicates the existence of a central localizing process distinct from the peripheral auditory processes of localization.

Both kinds of localization, we assume, were well developed at the start of the experiment. They were thoroughly integrated and well habituated. The pseudophone merely produced a split between the auditory and non-auditory factors in the localization of sound.

Habituation to the pseudophone, in the writer's experiment, led to increased reliance upon the non-auditory kind of localization. The practical necessity of finding one's way about busy streets when auditory localizations were known to be unreliable and a source of danger led to an increased reliance upon vision. This accentuated the line of cleavage between the two kinds of localization.

After habituation the writer was able to go for several hours on a busy street without noticing any marked disturbance in sound localizations. Sounds were heard in their normal places without any sense of effort; localizations were immediately normal and indistinguishable from those of everyday life. Only occasionally when a sound came suddenly or from an unknown source was a discrepancy discovered. Despite all of this, localizations were consistently reversed when tested in the laboratory without knowledge of the position of the source.

At the start there was considerable confusion, indefinite localization, dissociation of sight and sound, double localization, and other marks of discrepancy between visual and auditory space. After habituation the frequency of these disturbances appeared to be lessened.

When the pseudophone was removed at the close of the experiment there was an immediate and complete return to

normal sound localization. No significant after-effect of habituation could be discovered. This is precisely what one would expect upon the assumption that the pseudophone produced a functional split between two kinds of localization both of which were well developed through years of experience before the experiment.

Carr (1, 149) has stated that in the pseudophonic experiment there was a reorganization of localizing habits, and Willey, Inglis and Pearce have questioned this interpretation. To the writer it seems that the psychological situation with the pseudophone is very different from that in the experiments of Stratton, Wooster, and Ewert, and from that in the well-known mirror-drawing experiment. In these experiments there is the need to develop *new* motor coördinations. In the pseudophonic experiment, by contrast, there was an artificial split between two well-established and normally integrated localizing operations. Habituation accentuated the cleavage, revealed the two mechanisms with increasing clearness, and led to increased reliance upon one system without developing any radically new system of motor coördination.

It is true that the pseudophone produced incorrect motor adjustments and movements, confusion, and other signs of disorientation. But with habituation these became less obtrusive and less troublesome.

## Conclusion

A comparison of the experiment of Willey, Inglis and Pearce with that of the writer upon reversed auditory localization, reveals that the two are in excellent agreement so far as the effect of the pseudophone upon purely auditory localization is concerned. The two studies differ in their view of the nature and development of normal sound localization through visual-motor adjustment. A reconsideration of all available evidence leads to the view that the pseudophones produced an artificial separation between two kinds of sound localization, auditory and non-auditory. Both kinds of localization were well developed and integrated at the start of the experiment. Their separation resulted in a

disorientation which tended, however, to become less disturbing and less noticeable under the necessity of increased reliance upon vision.

## REFERENCES

1. Carr, H. A., An introduction to space perception, New York: Longman's, 1935.
2. Ewert, P. H., A study of the effect of inverted retinal stimulation upon spatially coördinated behavior, *Genet. Psychol. Monog.*, 1930, 7, 177–363.
3. Klemm, O., Untersuchungen über die Lokalisation von Schallreizen, *Arch. f. d. ges. Psychol.*, 1918, 38, 94–5.
4. Stratton, G. M., Vision without inversion of the retinal image, Psychol. Rev., 1896, 3, 611–617; 1897, 4, 341–360, 463–481.
5. Willey, C. F., Inglis, E. and Pearce, C. H., Reversal of auditory localization, *J. Exper. Psychol.*, 1937, 20, 114–130.
6. Wooster, M., Certain factors in the development of a new spatial coördination, *Psychol. Monog.*, 1923, 32, No. 4.
7. Young, P. T., Auditory localization with acoustical transposition of the ears, *J. Exper. Psychol.*, 1928, 11, 399–429.
8. ——, The rôle of head movements in auditory localization, *J. Exper. Psychol.*, 1931, 14, 95–124.

[MS. received May 20, 1937]

# ON GESTALT-QUALITIES*

BY CHRISTIAN VON EHRENFELS

(TRANSLATED BY MILDRED FOCHT)

The theory of Gestalt-qualities began with the attempt to answer a question: What is melody? First and most obvious answer: the sum of the individual notes which make up the melody. But opposed to this is the fact that the same melody may be made up of quite different groups of notes, as happens when the self-same melody is transposed into different keys. If the melody were nothing else than the sum of the notes, different melodies would have to be produced, because different groups of notes are here involved. Mach, who was struck by this fact, drew from it the conclusion that the essence of melody must reside in a sum of special sensations which as note-sensations (*Tonempfindungen*) accompany the notes. But he did not know how to give any account of these sensations; and in fact we are unable to discover any sign of them in inner experience. The decisive step in the founding of a theory of Gestalt-quality was my own assertion: when the memory-images of successive notes are present as a simultaneous complex in consciousness, then an idea (*Vorstellung*), belonging to a new category, can arise in consciousness, a unitary idea, which is connected in a manner peculiar to itself with the ideas (*Vorstellungen*) of the complex of notes involved. The idea of this whole belongs to a new category, for which the name 'founded contents' (*fundierte Inhalte*) came into use. Not all founded contents are perceptual in character and allied to the idea of melody. There are also founded contents which are not perceptual, as for example relations. The essence of the connection between the founded

* Dictated to his wife some weeks before his death in 1932, and now published for the first time.

content and its base (*Fundament*) is the irreversible conditioning of the former by the latter. Every founded content necessarily requires a base. A certain complex of basic ideas can support only a certain founded content. But not every base need be crowned, so to speak, and held together, by a founded content. At least that was my view when I formed the concept of Gestalt-quality. Others held a different view, which was, that the Gestalt-quality is necessarily given along with the base, and that the effort which we contribute—for instance, in apprehending a melody—is not located in production of the founded content, but merely in its observation. Meinong and his pupil Benussi adhered to the first view, while the second is represented by Wertheimer and Köhler.

Gestalt-qualities may be divided into apprehension (*Auffassung*) of processes and apprehension of momentary states. I have distinguished these groups as temporal and non-temporal Gestalt-qualities. Examples of processes are melody and motion; of momentary states, harmony, and what in everyday life is designated spatial structure (*Raumgestalt*). But there are not only melody and harmony of sound, but also, for example, color-melodies and color-harmonies as well. Indeed, in the realm of all sense-qualities it is possible to find analogues of melody and harmony. But the field of Gestalten extends far more widely than is shown by these examples. In the first place there is Gestalt not only in the realm of musical notes, but also in that of the sound-sensations which we call 'noises' (*Geräusche*) in order to distinguish between musical notes and mere sounds. Language is composed of such noises, even if in the individual vowels, as elements, tones are distinguishable. Every word of a language is a Gestalt-quality. As to the extension of Gestalt-qualities into psychical life, one can form an idea from the fact that the so-called laws of association operate much more frequently when Gestalten and not elements are concerned. Thus, for instance, with the image of an individual person is associated (certainly physically and in all probability psychically) a Gestalt-quality, which, according to the law of similarity, is numerous images of other persons; while with the

idea of a simple element, for example of a musical note or a color, the idea of other elements is never associated. Our memory for simple elements in the realm of musical notes, the so-called absolute pitch, is incomparably less developed than the memory for melodies and harmonies. The so-called mnemonic aids are based on Gestalt-qualities. The essence of the aids consists in this, that a Gestalt-quality is discovered which for some reason or other is easily imprinted on the memory and whose parts have a certain mechanical relation to the ideational objects which are to be retained in memory: as for example the often-cited phrase, readily accepted by the ear: "Kilometertal, Euer Urpokal," in which the first two syllables are readily associated with the name Klio, and every following syllable is identical with the first syllable of the name of one of the nine muses.

Belief in Gestalt-qualities also forms the basis of my cosmogony. In order to understand this point of view, it is necessary to distinguish higher and lower Gestalten from each other.

Every determinate body has some kind of Gestalt. But he who compares the Gestalt of a clod or of a heap of stones with the Gestalten of a tulip or a swallow will at once have to admit that the tulip or the swallow has realized the particular genus *Gestalt* in greater degree than the clod or the heap of stones. In like manner, all visible objects have some kind of color. Still, every unprejudiced person will agree that a brilliant red is color to a greater degree than gray is. But while there is nothing, except the visual impression, to which reference can be made in order to describe this characteristic of 'being color to a greater degree' (*Mehr-Farbensein*), higher Gestalt as opposed to lower can be determined very well by distinguishing marks. Higher Gestalten are those in which the combined effect of unity of the whole and manifoldness of the parts (*das Produkt von Einheitlichkeit des Ganzen und Mannigfaltigkeit der Teile*) is greater. To give the simplest possible examples: if in regular polygons all possible diagonals are drawn, Gestalten of approximately similar unity are produced. If we begin with the square and advance to the

pentagon, we obtain figures which qualify as higher and lower Gestalten in proportion to the number of their parts. But if we begin, for instance, with an irregular eight-angled polygon, and construct a series of polygons which approximate a regular octagon more and more closely, the Gestalten of these polygons with their diagonals will become higher, the closer the irregular ones approach to the regular one.

[MS. received March 19, 1937]

# TOLMAN ON ASSOCIATIVE LEARNING

BY E. R. GUTHRIE

*University of Washington*

Professor Tolman has recently published an article in the PSYCHOLOGICAL REVIEW entitled 'The acquisition of string-pulling by rats—Conditioned response or sign-Gestalt?,' and in it an experiment by McCulloch and some observations of Tolman's own are subjected to two analyses, one in terms of conditioning and one in terms of sign-gestalt-expectation. The rat-subjects were found to conform to the sign-gestalt description and not to the description in terms of associative learning.

In the course of the experiment the rats learned to pull in a food-pan by means of a string and so to obtain food. It is this learning which is analyzed in terms of the two theories. The writer is taking the liberty of suggesting a radically different stimulus-response analysis.

*Before training has taken place* the behavior which can be expected of the rat judging from Tolman's own account is as follows:

1. If hungry the rat will eat food from the pan if this is placed before him.

2. If the pan is gently removed while the rat is eating and its foot is on the edge of the pan, the rat will pull back on the pan.

3. If the pan is pulled farther under the same conditions, the rat will 'scrabble' and catch the string in mouth or paw.

4. The rat will pick up a string on which food is smeared.

5. It will seize and pull on the string if the string is 'jiggled.'

6. If the pan is out of reach, the rat *will not* without certain training pull in on the string and obtain food.

*After the training has taken place* the rat will, if hungry, pull in the string until the pan is within reach and proceed to eat.

What must be accounted for by a theory of learning is this difference between the behavior of the rat before and after training.

Training consists in first allowing a hungry rat to eat food from the pan and then, while it is eating, drawing the pan slowly away in such a manner as to induce the rat to clutch the pan and draw it back. This the unsophisticated rat can be counted on to do. After this has been done several times, the pan with string attached is drawn slowly out of reach of the rat while the rat is 'scrabbling' for the pan. The rat will then sometimes paw the string and pull or take the string into its mouth and pull. By increasing gradually the distance to which the pan is removed the rat will eventually pull the pan in from a distance of 50 cm.

In a very curious stimulus-response analysis of this series of events Tolman states that *the* unconditioned stimulus is food in the pan, that *the* unconditioned response is eating, that *the* to-be-substituted stimulus is the string, and that *the* conditioned response is seizing and pulling. He then points out that "the learned (*i.e.*, the supposedly conditioned) response of pulling the string is not, as it seems to me [Tolman] it should be according to a strict conditioned response interpretation, either the original unconditioned response of eating, or a response 'closely' related to the latter."

Actually we are here dealing with at least five 'unconditioned' stimuli and their responses, the five listed above. These five are, of course, 'unconditioned' only in the sense that cue and response have been already connected by association and can be depended on when the experiment begins. My objections to the emphasis on an 'unconditioned' stimulus have been stated elsewhere. The 'unconditioned' stimulus is never the sole 'cause' of the response, but only the convenient final touch by which the response can be released under the conditions of an experiment. The rat must be hungry, active, not engaged in fighting, he must be right-side-up, and so on. It is impossible and impractical to enumerate all the stimulus elements which co-operate in any response. We usually take most of them for granted.

What needs to be explained is how the animal comes to pull on the string when hungry and confronted with the string and with food-box out of reach.

A conditioned response explanation would only point out that by enticing the rat to pull under these circumstances the rat will later pull under these circumstances without the enticement. Jiggling the string, pulling on the box while the rat's foot was on the edge, smearing the string with food, all these were just inducements to get the rat to pull on the string. These were what may be called the unconditioned stimuli. After a little of this, the rat pulls on seeing the string.

The rôle played by the reward is a complex one, but not too complex to be understood if one is so disposed. If the string were very long, fatigue and distraction would eventually cause the rat to cease pulling and the association of string and pull would be dis-established. With the string still present the rat would be doing something else and so there would be a conditioned inhibition of the string-pulling.

If the rat is allowed to eat to satiety, hunger disappears, and there is no chance for a conditioned inhibition of pulling as a response to hunger plus sight-of-string. Hunger is now absent. When it recurs together with the sight of the string, the rat will pull again. This may be weeks later.

If the rat is not allowed to eat to satiety, the nibble of food starts salivation and movements of eating; these will surely add to the drive and the 'integration' of the rat's behavior, and some saliva flow and chewing movements may survive to the next trial. The rat will now pull with a set to eat. By set is meant nothing more mysterious than saliva flow and eating movements such as are not incompatible with the absence of food. In this sense the string becomes what Tolman calls a 'sign-gestalt-expectation-hypothesis.' And it really does. If Tolman will be careful not to attribute to animals expectations that involve language, I have no objection to his use of the word. Not that I shall use it myself.

For Tolman this sign- . . . -hypothesis is an 'intervening variable.' In order to get on to any actual movement on the

part of the rat he devises a new principle of learning (number 4): "The outward expression of such an acquired set (expectation) is that the animal (given the proper conditions of motivation) tends to behave to the more immediate stimulus-object in either the way to get to or the way to avoid such more distant stimulus-objects." In contrast to this, the stimulus-response description in terms of associative learning predicts the consequent behavior directly without recourse to this intervening variable.

If this article has conveyed the impression that the writer finds the sign-gestalt description incorrect, that was not its intention. Things do become signs of things to come. All of the seven kinds of learning listed by Tolman do occur, and many other kinds as well. We all learn at times to react to a signal with behavior appropriate to what is to follow. This is Tolman's 'signal learning' and is number 2 of his list. In my own case I have also observed numerous learned reactions to signals, but reactions which were highly inappropriate. For this I can find no provision in Tolman's list, which seems to apply only to the behavior of very nice and very wise people. The description of my wrong acts in terms of association seems to me, at least, to be as a rule very apt.

[MS. received May 15, 1937]

# H. A. CARR ON THE PROBLEM OF RELIABILITY

BY WALTER S. HUNTER

*Brown University*

Carr's recent paper on "The search for certainty" in this journal (2) resumes and extends the discussion of reliability which he published in 1926 (1), a discussion in which I also shared (5). Inasmuch as Carr's last paper fails to present correctly my own position on the problem and fails to consider recent publications in the field where ideas similar to his own are developed, it seems worth while to place certain addenda on record in the same volume for the benefit of those readers who have not followed the topic in detail and who might naturally believe that my students and I had committed the sins enumerated by Carr. (I am happy to receive credit for my share in initiating and in somewhat shaping the development of studies of the reliability of the maze, but I disclaim responsibility for much that has been written from other laboratories!)

Carr writes: "Under this title I propose to discuss what I regard as an illegitimate method of measuring reliability in the field of learning. I refer to the method of measuring reliability by correlating the scores of the even and odd trials, and of assuming that the degree of reliability varies directly with the size of this correlation." ". . . Hunter secured records upon a number of problems, and computed the correlations between the even and odd trials" (2, p. 274).

As a matter of fact, so far as I can discover, *only once* have I used (and then but incidentally) the method of odd and even trials. This occurred in an article published with Randolph in 1924 (6) where among many other items mention was made of two odd *vs* even trial correlations. My first study (1922) bearing specifically on this general topic was entitled 'Correlation Studies With the Maze in Rats and Humans' (4). Here I correlated various (Vincent) tenths of

learning (but not odd *vs* even ones) with the purpose not of directly determining reliability but of discovering the inter-relationships of parts of the learning process to each other. Heron's work (3) in 1922 was the only study in my laboratory until 1932, except for the one minor instance in the Hunter-Randolph paper, where odd and even trials were correlated. This was done, even in Heron's study, for completeness of approach to the problem; and an examination of his paper will show that emphasis was placed on correlations between test and retest, between the first part and all of learning, and between total scores on the maze and on the problem box. Two further studies (7 and 8) came from my Clark laboratory from 1932 through 1936. Here the method of odd *vs* even trials was involved, but the method was used for strictly critical purposes.

My own attitude that the problem of reliability of total learning scores is best approached through the determination of test-retest correlations dates from the preliminary work which Paterson did (1917) at Kansas, work which was in no sense an uncritical taking over of mental test methods. This attitude was reflected in Heron's study and in the remainder of the Kansas experiments. In contrast to the test-retest reliability method, correlations of odd and even trials or of different parts of the learning process I have thought of as determinations of internal consistency in the learning records. This view is clearly expressed in Leeper's (6) work published in 1932 from my laboratory, particularly pages 161–166. In these pages Leeper indicates the dubiousness of applying the Spearman-Brown formula to odd and even trial correlations as a method of determining the reliability of the total maze score. Not being content to have this excellent work stand alone, I encouraged Newman to carry the problem forward. This he did in two articles on 'The Determination of the Reliability of Learning Material and the Spearman-Brown Formula' published in 1936 (7). Newman not only endorsed Leeper's discussion but extended it to odd *vs* even culs-de-sac. He stressed the inter-dependence of parts (trials or culs) as fundamentally opposed to the basic assumptions

of the Spearman-Brown formula, and he went on to an experimental check of the predictive value of the formula when applied in human and animal learning.

Carr leaves me with the impression that such points have not been raised and treated, either theoretically or experimentally, when he writes as follows without citing previous work: ". . . we here encounter the fundamental fallacy involved in applying this method [odd *vs* even trials] to learning data. What is the learning process? It is a process in which the performance of an individual in the tenth trial is a function of the preceding trials. It is a process in which the individual's performance changes from trial to trial in virtue of the preceding trials. It is a process in which the scores for the successive trials are so causally tied together that the scores for the odd and even trials must necessarily exhibit some sort of correlation with each other" (**2**, pp. 286–287).

In 1936 Newman (**8**, p. 2) wrote: "It is evident, theoretically, that measures of learning are much more interdependent than are the measures obtained by means of the usual mental testing procedures. Due to the fact that investigators in the field of learning have attempted to apply the methods used to determine the reliability of test material to the determination of the reliability of learning material, the methods as applied to learning material are in need of experimental analysis in the light of the above theoretical considerations." Four years before, in 1932, Leeper had said, among other things, the following: "However this formula (Spearman-Brown) is peculiarly dependent on the assumption that the added material is of equal difficulty, and independent of the material to which it is added. And as has been indicated before, it seems certain that the various parts of one training series do *not* have this independence" (**7**, p. 165). This same point was made independently in the same year by Spence (**9**) whose excellent monograph gives most serious consideration to the various interpretative problems in the determination of reliability.

I regret with Carr the extensive use of the method of correlating odd and even trials and the use of the Spearman-Brown formula as a means of determining the reliability of measures of learning. Both procedures are unjustifiable as methods of determining the reliability of total learning scores. Carr is right in so far as he condemns them.

REFERENCES

1. CARR, H. A., The reliability of the maze experiment, *J. Comp. Psychol.*, 1926, **6**, 85–93.
2. ——, The search for certainty, PSYCHOL. REV., 1937, **44**, 274–296.
3. HERON, W. T., The reliability of the inclined plane problem box as a method of measuring the learning ability of the rat, *Comp. Psychol. Monog.*, 1922, **1**, 1–36.
4. HUNTER, W. S., Correlation studies with the maze in rats and humans, *Comp. Psychol. Monog.*, 1922, **1**, 37–56.
5. ——, A reply to Professor Carr on 'The reliability of the maze experiment,' *J. Comp. Psychol.*, 1926, **6**, 393–398.
6. —— AND RANDOLPH, V., Further studies on the reliability of the maze with rats and humans, *J. Comp. Psychol.*, 1924, **4**, 431–442.
7. LEEPER, R., The reliability and validity of maze experiments with white rats, *Genet. Psychol. Monog.*, 1932, **11**, 137–245.
8. NEWMAN, S. H., The determination of the reliability of learning material and the Spearman-Brown formula, *J. Comp. Psychol.*, 1936, **22**, 1–60.
9. SPENCE, K. W., The reliability of the maze and methods of its determination, *Comp. Psychol. Monog.*, 1932, **8**, 1–45.

[MS. received July 14, 1937]

# A NOTE ON VERIFICATIONS OF THE JUDGMENTAL THEORY OF PLEASANTNESS AND UNPLEASANTNESS

BY HENRY N. PETERS

*University of Missouri*

In the writer's exposition of Carr's judgmental theory of feeling (1, p. 383), he outlined a possible experimental test of the theory. This hypothetical experiment was in brief described as follows. A learning problem is arranged in which a number of distinctly separate stimulus items arouse positive or negative reactions. In the eventual solution of the problem, each stimulus item arouses either a consistently positive or a consistently negative reaction. These stimulus items are to be judged as to hedonic tone both before and after the learning. The verification of the theory to be sought is a comparison of the relative feeling tones before and after. If it were found that the stimuli arousing positive reactions moved in the direction of *P* and those arousing negative reactions in the learning problem moved toward *U*, the theory would be verified.

Until recently the writer was unaware that any experiments even approximating this hypothetical one had been performed. There are, however, in the *Psychological Abstracts*, two abstracts of papers published in Russian which describe experiments almost identical with the one outlined above, although the purpose of the studies was apparently not that of testing the judgmental theory.[1]

Young children were used as subjects in these experiments. They were first required to tell which of a series of colors they liked and which they disliked. Then each child was experi-

[1] *Psychological Abstracts*, 9, 1935; the Russian articles are by L. E. Khozak and V. Kovsharova and have been abstracted by G. H. S. Razran under numbers 1140 and 1144 respectively.

mentally conditioned to colored lights by presenting it with food (*e.g.*, chocolate) always with the disliked colors—thus arousing approach reactions—and by withholding food or presenting the child with less preferred food (*e.g.*, biscuits) with the liked colors—thus arousing avoiding reactions, inhibition of positive reactions, or simply no reaction. After this conditioning the children again rated the colors and in most cases there was a complete inversion of the relative hedonic tones.

The particularly valuable feature of these experiments, one which they share with conditioning studies in general, is that they lay bare the essential machinery of learning. Now these machinations in the determination of our everyday likes and dislikes seem most obvious. And much anecdotal data relevant to pleasantness and unpleasantness, as well as many laboratory studies take on a new light. Examples of these latter, which now seem to be a perfect verification of the judgmental theory, are reported by Thorndike in his 'The Psychology of Wants, Interests and Attitudes' (**2**).

It is equally clear in Thorndike's studies and in the abstracts described that it is learning which conditions pleasantness and unpleasantness, and not the other way around. While the evidence for ideational reactions, remembering and forgetting, is not so clear as that for sensory-motor reactions, the writer proposes that the same dependency found for the one type should be assumed most likely for the other until it is proved otherwise. Since the majority of recent studies of the relationship between memory and feeling tone have explicated the opposite dependent relationship, *i.e.*, that memory depends upon pleasantness and unpleasantness, this proposition is not manifest.

If a rat were capable of symbolic reactions he would label the cues to which he had differentially reacted in learning a maze as *P* and as *U*. Those which he had approached in following the true path would be *P*; those which he had avoided in keeping out of culs-de-sac would be *U*. However, since rats have never been shown to make reactions in a purely symbolic sense (judgmental), they must not at present be

credited with experiences of *P* and *U*. Apparently the meaning of this last statement is also not manifest.

## REFERENCES

1. PETERS, H. N., The judgmental theory of pleasantness and unpleasantness, PSYCHOL. REV., 1935, 42, 354–386.
2. THORNDIKE, E. L., The psychology of wants, interests and attitudes, New York: D. Appleton-Century Company, 1935, chapter 16.

[MS. received March 15, 1937]

# PSYCHOLOGICAL REVIEW PUBLICATIONS

Original contributions and discussions intended for the Psychological Review should be addressed to

**Professor Herbert S. Langfeld,** Editor PSYCHOLOGICAL REVIEW,
Princeton University, Princeton, N. J.

Original contributions and discussions intended for the Journal of Experimental Psychology should be addressed to

**Professor Samuel W. Fernberger,** Editor JOURNAL OF EXPERIMENTAL PSYCHOLOGY,
University of Pennsylvania, Philadelphia, Pa.

Contributions intended for the Psychological Monographs should be addressed to

**Professor John F. Dashiell,** Editor PSYCHOLOGICAL MONOGRAPHS,
University of North Carolina, Chapel Hill, N. C.

Reviews of books and articles intended for the Psychological Bulletin, announcements and notes of current interest, and *books offered for review* should be sent to

**Professor John A. McGeoch,** Editor PSYCHOLOGICAL BULLETIN,
Wesleyan University, Middletown, Conn.

All business communications should be addressed to

**Psychological Review Company,** Ohio State University, Columbus, Ohio

---

## THE PSYCHOLOGICAL REVIEW

is indexed in the

## International Index to Periodicals

to be found in most public and
college libraries

# AMERICAN PSYCHOLOGICAL PERIODICALS

**American Journal of Psychology**—Ithaca, N. Y.; Cornell University.
Subscription $6.50. 624 pages annually. Edited by K. M. Dallenbach, M. F. Washburn, Madison Bentley, and E. G. Boring.
Quarterly. General and experimental psychology. Founded 1887.

**Journal of Genetic Psychology**—Provincetown, Mass.; The Journal Press.
Subscription $14.00 per yr.; $7.00 per vol. 1,000 pages ann. (2 vols.). Edited by Carl Murchison.
Quarterly. Child behavior, animal behavior, comparative psychology. Founded 1891.

**Psychological Review**—Ohio State University, Columbus; Psychological Review Company.
Subscription $5.50. 540 pages annually. Edited by Herbert S. Langfeld.
Bi-monthly. General psychology. Founded 1894.

**Psychological Monographs**—Ohio State University, Columbus; Psychological Review Company.
Subscription $6.00 per vol. 500 pages. Edited by John F. Dashiell.
Without fixed dates, each issue one or more researches. Founded 1895.

**Psychological Bulletin**—Ohio State University, Columbus; Psychological Review Company.
Subscription $6.00. 720 pages annually. Edited by John A. McGeoch.
Monthly (10 numbers). Psychological literature. Founded 1904.

**Archives of Psychology**—New York, N. Y.; Columbia University.
Subscription $6.00. 500 pages per volume. Edited by R. S. Woodworth.
Without fixed dates, each number a single experimental study. Founded 1906.

**Journal of Abnormal and Social Psychology**—Ohio State University, Columbus; American Psychological Association.
Subscription $5.00. 560 pages annually. Edited by Gordon W. Allport.
Quarterly. Abnormal and social. Founded 1906.

**Psychological Clinic**—Philadelphia, Pa.; Psychological Clinic Press.
Subscription $3.00. 288 pages. Edited by Lightner Witmer.
Without fixed dates (Quarterly). Orthogenics, psychology, hygiene. Founded 1907.

**Journal of Educational Psychology**—Baltimore; Warwick & York.
Subscription $6.00. 720 pages. Monthly except June to August.
Edited by J. W. Dunlap, P. M. Symonds and H. E. Jones. Founded 1910.

**Psychoanalytic Review**—Washington, D. C.; 3617 10th St., N. W.
Subscription $6.00. 500 pages annually. Edited by W. A. White and S. E. Jelliffe.
Quarterly. Psychoanalysis. Founded 1913.

**Journal of Experimental Psychology**—Ohio State University, Columbus; Psychological Review Company.
Subscription $14.00 (2 vols.). 1250 pages annually. Edited by Samuel W. Fernberger.
Monthly. Experimental psychology. Founded 1916.

**Journal of Applied Psychology**—Indianapolis; C. E. Pauley & Co.
Subscription $6.00. 600 pages annually. Edited by James P. Porter, Ohio University, Athens, Ohio. Bi-monthly. Founded 1917.

**Journal of Comparative Psychology**—Baltimore, Md.; Williams & Wilkins Company.
Subscription $5.00 per volume of 450 pages. Ed. by Knight Dunlap and Robert M. Yerkes. Two volumes a year. Founded 1921.

**Comparative Psychology Monographs**—Baltimore, Md.; The Johns Hopkins Press.
Subscription $5.00. 400 pages per volume. Roy M. Dorcus, Editor.
Published without fixed dates, each number a single research. Founded 1922.

**Genetic Psychology Monographs**—Provincetown, Mass.; The Journal Press.
Subscription $7.00. 500 pages annually (1 vol.). Edited by Carl Murchison.
Quarterly. Each number one or more complete researches. Child behavior, animal behavior, and comparative psychology. Founded 1925.

**Psychological Abstracts**—Ohio State University, Columbus; American Psychological Association.
Subscription $7.00. 700 pages ann. Edited by Walter S. Hunter and R. R. Willoughby.
Monthly. Abstracts of psychological literature. Founded 1927.

**Journal of General Psychology**—Provincetown, Mass.; The Journal Press.
Subscription $14.00 per yr.; $7.00 per vol. 1,000 pages ann. (2 vols.). Edited by Carl Murchison.
Quarterly. Experimental, theoretical, clinical, historical psychology. Founded 1927.

**Journal of Social Psychology**—Provincetown, Mass.; The Journal Press.
Subscription $7.00. 500 pages annually. Ed. by John Dewey and Carl Murchison.
Quarterly. Political, racial, and differential psychology. Founded 1929.